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TO

FRANK GILL

PREFACE

These ten memoirs had their origin in a suggestion for which the author is indebted to Mr. J. L. M'Quarrie, that an account of Sir Charles Wheatstone, illustrated by references to relics of apparatus in the George III. Museum at King's College, London, would be an acceptable contribution to the history of the electricians. It was possible to follow that account by the story of Maxwell at the Cavendish Laboratory, Oersted at Copenhagen, Ohm at Munich, Hertz at Karlsruhe and Bonn, and by others of like character. Most of the memoirs were published in *Electrical Communication.*

Gratitude must here be expressed to innumerable helpers —in London, Cambridge, Torquay, Berlin, Lyons, Erlangen, Cologne, Munich, Nuremberg, Heidelberg, Karlsruhe, Jena, Bonn, Milan, Como, Florence, Bologna, Rome, Leyden, Delft, Haarlem, Amsterdam, Copenhagen, and Paris—who with zeal and friendliness allowed a stranger to wander amongst the treasures of their laboratories, homes, and libraries.

R. A.

October 17, 1929.

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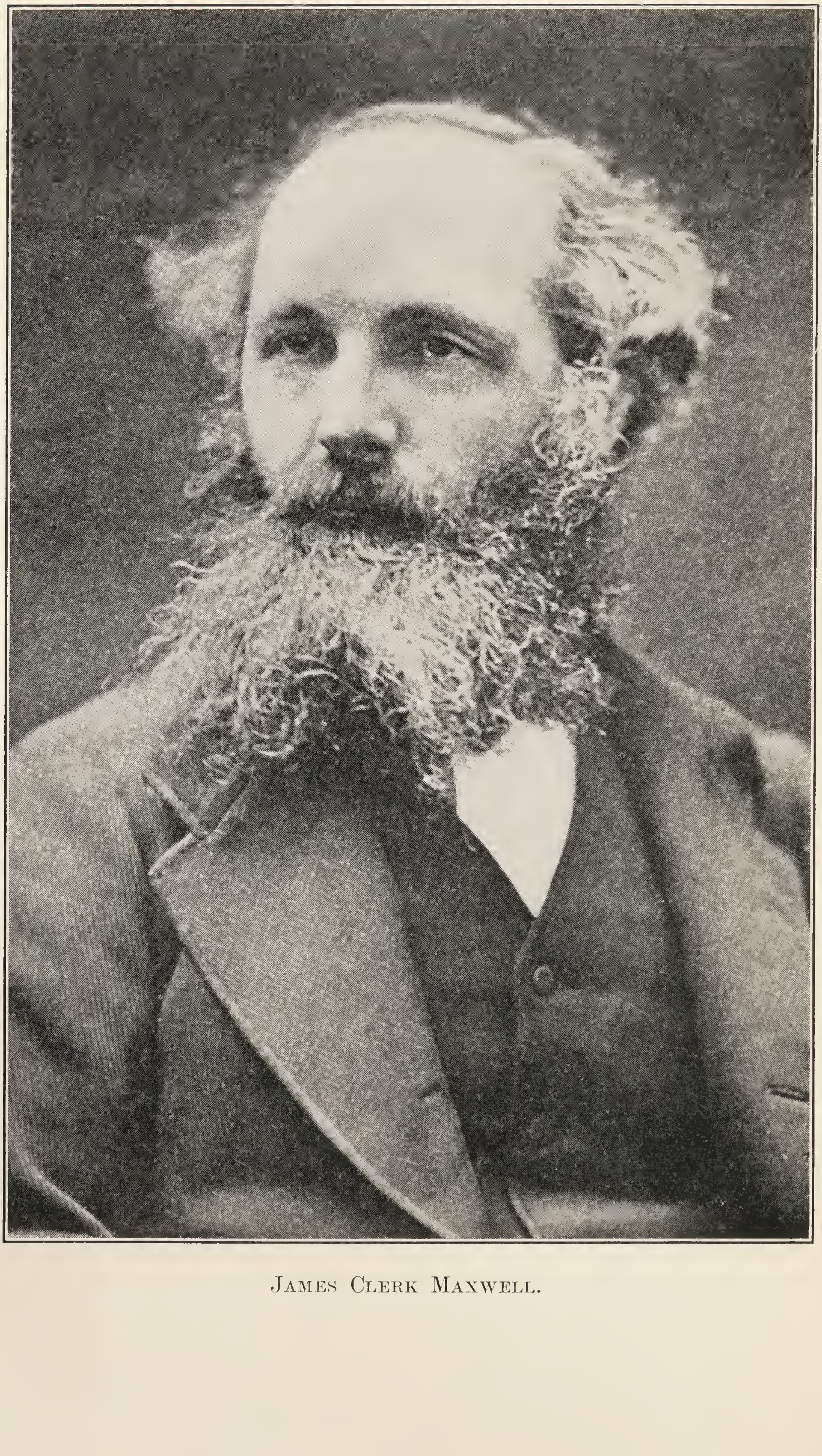


James Clerk Maxwell.

I

JAMES CLERK MAXWELL

Macaulay wrote of Newton that in no other mind have the demonstrative faculty and the inductive faculty co-existed in such supreme excellence and perfect harmony. Throughout the history of natural philosophy there is no name, except Newton's, more honoured by men of science than that of James Clerk Maxwell. It is difficult to compare the works of these two men, for Newton lived to 85, and Maxwell only to 48; yet they had much in common. To claim Maxwell as a pioneer of electrical communication is to direct attention to but a single field of activity in which his influence has hastened progress. Events, however, have proved it to be a field of sufficient scope to exemplify his labours in innumerable phases of research; for it was this territory among the mountain-tops of electrical theory relating to transmission that Clerk Maxwell surveyed. When he had constructed paths across it, mapped it, illuminated it, and had placed at its salients landmarks for future explorers, he left it to us as an inheritance.

For the last sixty years his contributions to theory have been an inspiration to the physicist. They remain authoritative, and a never-failing source of enlightenment. At first, their mathematical character was a barrier rarely crossed by engineers; but with the advance of applied science, and with the steady movement of physical research from academical to industrial centres, mathematics in the electrical laboratory has become an open book, and Maxwell's influence is now everywhere manifest. Hence, the story of Clerk Maxwell can no longer be limited to biographical details of his parentage, childhood, undergraduate life, friendships, and general career. An

attempt must be made to indicate something of his work and influence, and to obtain a hint of the circumstances that produced in him those transcending qualities that have crowned his work with permanence and his name with immortality. With this in view, it is best to contemplate first the broad outline of his whole career and of his achievements, and then to select, from what is known concerning him, such details as may help to reveal something of his mode of thought, the secret of his thoroughness, and of his perceptive faculties relating to the physical interpretation of Nature.

James Clerk Maxwell was born on June 13, 1831, at 14 India Street, Edinburgh. His father, John Clerk Maxwell, was of the family of the Clerks of Penicuik in Midlothian. His mother was the daughter of R. H. Cay, of Charlton, Northumberland. In addition to the usual characteristics of parents, his father took "persistent interest in all useful processes", and he inculcated upon his son the doctrine of knowing how things are done and how they work. Their home, Glenlair, was a country house and estate of modest dimensions, by the pleasant water of Urr, about a day's journey, at that time, from Edinburgh. In 1841 James was sent to Edinburgh Academy, where his schoolfellows, with the humour that makes Scotland what it is, nicknamed him "Dafty". His reply to them was to take the first place in English, the prize for English verse, and the Mathematical Medal. At the age of fourteen he made his first contribution to science in the form of a paper "on the description of Oval Curves and those having a plurality of Foci". This was ultimately printed, with remarks by Professor J. D. Forbes, in the *Proceedings of the Royal Society of Edinburgh*, for April, 1846. At fifteen, Clerk Maxwell, with enthusiasm derived largely from Forbes, was giving attention to chemistry, magnetism, and the polarization of light; and what was of equal importance, he was discussing these and kindred matters with his schoolfellow Peter Guthrie Tait. At sixteen he entered the University of Edinburgh, where he remained for three Sessions. There he attended the Logic Class of Sir William Hamilton, the founder of Quaternions, who developed his speculative faculties. Meanwhile he absorbed mathematics, and all that he could find in

relation to mechanics, optics, and the theory of heat. In October, 1850, he went up to Cambridge, taking with him, be it observed, his scraps of gelatine, gutta-percha, unannealed glass, his bits of magnetized steel, and other portions of matter dear to his peripatetic mind.

At Cambridge, he soon migrated from Peterhouse College to Trinity College. Adam Sedgwick (1785–1873), the renowned geologist, was then at Trinity. Sir George Gabriel Stokes (1819–1903), mathematician and physicist, was at Pembroke. Stokes had, in the years 1845–50, published important memoirs on the motion of viscous fluids, and he had investigated "with dynamical implications" Newton's coloured rings, diffraction, polarization, and the propagation of disturbances from vibrating centres. Clerk Maxwell attended Stokes's lectures, and they became intimate friends. In the words of Sir Joseph Larmor, in a biography of Stokes, the way was thus prepared for Clerk Maxwell's interpretation of Faraday, and for the modern wide expansion of ideas.

In the mathematical contest at Cambridge in January, 1854, Routh of Peterhouse appeared as Senior Wrangler and as Smith's Prizeman. Clerk Maxwell was Second Wrangler and was bracketed with Routh for Smith's Prize. When the stress of examinations was over, his attention at once reverted to optics and to the study of matter and dynamics. A year later, in a letter to his father, he wrote "I am reading Electricity and working at Fluid Motion". It was in October of that year that he gained his fellowship at Trinity. Soon afterwards he was appointed lecturer there in Hydrostatics and Optics. It is characteristic of him that, notwithstanding his advances, he regarded it desirable still to attend the lectures of Professor Willis on Mechanics. He was considering also the transformation of surfaces by bending, the quantitative measurement of mixtures of colours, and the cause of colour-blindness. Meanwhile he was co-ordinating his ideas relating to Faraday's Lines of Force. Then, as subsequently, his procedure was from clear notion to clear notion—symbols and equations were merely secondary aids to thought.

Clerk Maxwell entertained a dread that he might "crystal-

lize" at Cambridge. Against this he took two precautions: in 1856 he accepted the professorship of Natural Philosophy at Marischal College, Aberdeen, and in June, 1858, he married Katherine Mary Dewar. She was the daughter of the Principal of that College. He remained at Aberdeen until 1860, when he became professor of Physics at King's College, London. In 1865 as the result of illness, he withdrew to Glenlair, where he remained, except during short intervals, until February, 1871, when he was appointed to the Chair of Experimental Physics at Cambridge University. It is to be observed that his residence in London brought him into close fellowship with Faraday, and that his subsequent retirement to Glenlair gave him the opportunity to prepare the manuscript for his great work *Electricity and Magnetism.* It has also to be remembered that between the years 1851 and 1865 great advances had been made in submarine telegraphy, bringing with them innumerable problems and a wealth of data for Clerk Maxwell to interpret.

Clerk Maxwell's ideas were established primarily upon Newton, with regard to work, energy, and acceleration as applied to systems of bodies. Davy, Rumford, and Joule had disposed of the doctrine that work spent in friction was necessarily lost; they had proved that it could be transformed equivalently into other forms of energy. These principles were extended by Clausius, Helmholtz, Mayer, Rankine, and Kelvin. "Perpetual Motion" had been discarded, and observation, experiment, and measurement had come into their own. But Clerk Maxwell's mission was not merely to interpret by equations and quantitative tests the soundness or otherwise of this or that guess at the answer to the riddle of the universe. His purpose was the elucidation of matter, motion, and electricity. He pointed out that many problems in Nature, especially those in which the dissipation of energy comes into play, are not capable of solution by the principles of thermo-dynamics alone, but that in order to understand them "we are obliged to form some more definite theory of the constitution of bodies". When seeking to explain structure, he admitted the difficulty of accounting for the identity in the properties of a multitude of

bodies, each unchangeable in magnitude, and some separated from others by distances that astronomy attempts in vain to measure; but he agreed that "the idea of the existence of unnumbered individual things, all alike and all unchangeable, is one which cannot enter the human mind and remain without fruit".

Referring in an early paper to Kelvin's theory of vortex atoms, he asked: "What if these molecules, indestructible as they are, turn out to be not substances but mere affections of some other substance?" The truth is that although the principle of the Conservation of Energy had opened the way to a rational hypothesis concerning the constitution of matter, the proposals of W. Weber (Poggendorff's *Annalen*, vol. 73, 1848) concerning the nature of electricity had led to a condition of stimulating perplexity.

Clerk Maxwell (*Transactions of the Cambridge Philosophical Society*, vol. x., part i., 1855) decided neither to be drawn aside by analytical subtleties, nor to be carried beyond the truth by any alluring hypothesis, but steadily to plod along the road of Faraday's experiments, and in particular to study Faraday's Lines of Force. He hoped at least to find a temporary theory that should guide other experimenters "without impeding the progress of the true theory when it appears". Then as now, more was known concerning the laws that appertain to matter, motion, and energy than about the constitution of the stuff that seems to be everything and that may be nothing.

It is helpful at this stage to realize how completely he recognized the two-fold character of the task that is set before the man of science in the realm of physics. Let us not confuse here the issue by attempting to distinguish between mathematicians, physicists, chemists, or logicians. He saw that "the work of mathematicians is of two kinds, one is counting, the other is thinking. Now these two operations help each other very much, but in a great many investigations the counting is such long and hard work that the mathematician girds himself to it as though he had contracted a heavy job, and thinks no more that day." He regarded thinking as "a nobler though more expensive occupation" than counting.

As an undergraduate he had described Cambridge mathematics as rather elementary. Later (1872) his opinion was:

> . . . Algebra is very far from O.K. after some centuries, differential calculus is in a mess, and is equivocal at Cambridge with respect to sign. We put down everything, payments, debts, receipts, cash credits, in a row or column, and trust to good sense in totting it up. . . . I am going to try to sow quaternion seed at Cambridge. . . . May one plough with an ox and an ass together?

His chief complaint was against "insufficient interpretation —letting your equations lead you by the nose". To his mind the physical universe was to be interpreted not in directionless symbols that denote mere quantities, inert and dead, but in vector terms that allow any defined material system to be thought about in respect to the relative positions of its parts, the directions of their velocities, the stresses between the parts, and generally in terms that aid in representing the eternal conspiracy between change of configuration, matter, and motion appertaining to defined material systems. "What", he asked, "is the most general specification of a material system consistent with the condition that the motions of those parts of the system which we can observe are what we find them to be?" For many purposes of physical reasoning he thought it desirable to fix the mind at once on a point of space instead of upon its three co-ordinates, and on the magnitude and direction of a force instead of on its three components. He gave full credit to Lagrange, to Hamilton, to Kelvin, and to Tait for their pioneer work in establishing appropriate dynamical concepts. It was to the task of arriving at logical consequences in general terms that he applied his penetrative and creative genius. He also developed strongly what was described by Tait as "the habit of constructing a mental representation of every problem". He thought of realities.

Of his many triumphs, probably the chief was the publication of his account of his Dynamical Theory of the Electromagnetic Field (*Royal Society Transactions*, vol. clv.; received October 27, 1864; read December 8, 1864). By that time, he seems to have discarded the hypothesis of molecular vortices. He applied the principle of energy to investigate the properties

of the medium. He supposed statical electricity, electromagnetic attractions, the induction of currents, and diamagnetic phenomena, to be produced by actions which go on in the surrounding medium as well as within the excited bodies, and he explained the action between distant bodies without assuming the existence of forces capable of acting directly at sensible distances. It was a medium through which light and heat could be transmitted, it could store the energy of motion, it could also store the energy of elastic resilience, it possessed inertia, and through it waves could be propagated.

He then applied his equations to the case of a magnetic disturbance propagated through a non-conducting field, and he showed that the only disturbances that can be so propagated are those which are transverse to the direction of propagation, and that the velocity of propagation is the velocity found by experiment to express the number of electrostatic units of electricity in one electromagnetic unit. He proceeded:

> This velocity is so nearly that of light that it seems we have strong reason to conclude that light itself—including radiant heat, and other radiations if any—is an electromagnetic disturbance in the form of waves propagated through the electromagnetic field according to electromagnetic laws. . . . If the same character of the elasticity is retained in dense transparent bodies, it appears that the square of the index of refraction is equal to the product of the specific dielectric capacity and the specific magnetic capacity.

Thus by contemplating the electromagnetic field, and by accepting Ohm's law as a cardinal principle, he established the electromagnetic theory of light, and deduced all the known laws of electricity and magnetism.

The importance of defining the term "resistance" was fully appreciated by him. The verification of Ohm's Law for metallic conductors by Chrystal (*British Association Report*, 1866, p. 36), was a further necessary step in the advance.

Maxwell wrote:

> There are no landmarks in space; one portion of space is exactly like every other portion, so that we cannot tell where we are. We are, as it were, on an unruffled sea, without stars, compass, soundings, wind, or tide, and we cannot tell in what direction we are

going. We have no log which we can cast out to take a dead reckoning; we may compute our rate of motion with respect to the neighbouring bodies, but we do not know how these bodies may be moving in space. . . . Energy cannot exist except in connection with Matter.

The greatness of Maxwell consists therefore in this, that out of confusion he made order, out of conjecture he moved towards certainty; and having developed at last a helpful theory, he demonstrated its validity by establishing from it the relationship between electricity, magnetism, and light. He may be said to have founded a dynasty of natural philosophy following that of Newton and succeeded by that of modern physics. To judge of his significance, he must be placed in that long dynastic line, a central figure of a mighty group, and his influence must be sought along the two roads, Before and After, that lead respectively to and from his achievements.

Before, came Tycho Brahe the observer, John Kepler and Galileo the co-ordinators, and Newton who associated matter and motion with time, distance, velocity, acceleration, mass, and force, in quantitative terms. In those happy days the fixed stars were at rest in a frame of adamant that was rigidly bolted down upon the concrete of eternity. Any point upon that frame could be selected as a datum of time and space for a rotating mechanical system of bodies. This frame served for Newton and it served for Maxwell, with reservations. Huygens was content with it, and so perhaps was Young when, in 1801, he revived the wave-theory of light. For these last, the frame was filled with aether, a type of matter that could undulate and that obeyed Newtonian laws. Except for its undulations, this aether was at rest in the motionless frame.

Clerk Maxwell's aether was not merely "luminiferous". He had to show in what manner stresses within it could produce electric and magnetic effects. On this account, he realized that in establishing a theory of electricity and magnetism he had to deal with internal relations more complex than those of any other science examined up to his time. His method of attack was to scrutinize all the known phenomena, to examine how they could be subjected to measurement, to trace the mathe-

matical relationships of the quantities measured, to compare the mathematical forms with those of dynamics, to deduce the most general conclusions possible from the data, and to apply the results to simple cases. He began with Faraday's researches because Faraday had seen lines of force where mathematicians had seen centres of force acting at a distance, because Faraday had seen a medium where they had seen nothing but space, because Faraday had sought the seat of the phenomena in that medium, while they were content when they had found it in a power of action at a distance impressed on electric fluids, because Faraday had begun with the whole and had arrived at the parts by analysis, while they had begun with the parts and had endeavoured to build up the whole by synthesis.

Electricity was admitted by Clerk Maxwell to the rank of a physical quantity, but he warned us that we must not too hastily assume it to be a substance, or to be a form of energy. All that he would regard as having been proved was that electricity could not be created or annihilated. In his theory, an electrified system was said to have a certain amount of energy, which could be calculated by multiplying the quantity of electricity in each of its parts by the "potential" of that part, and taking half the sum of such products. The resultant intensity at any given point of the medium surrounding a given point-charge of electricity was proportional to the charge divided by the square of the distance between the given point and the charge. This resultant intensity was accompanied by a "displacement of electricity" in a direction say outwards from the charge. The term "displacement" has occasionally led to misapprehension. It must be remembered that he was concerned with the language of Faraday, who adopted lines of force where Clerk Maxwell would have preferred lines of induction. The whole phenomenon of attraction or repulsion between two electrified bodies, when both bodies are contemplated, he called stress—a transference of momentum from one body to another. With him the mechanical action between two charged bodies is a stress, and that on one of them is a force. The force on the point-charge is proportional to the charge.

To exemplify his method of presenting profound truths in

plain language, it suffices to recall his statement of the four theorems:

I. If a closed curve be drawn embracing an electric current, then the integral of the magnetic intensity taken round the closed curve is equal to the current multiplied by 4π.

II. If a conducting circuit embraces a number of lines of magnetic force, and if from any cause whatever the number of these lines is diminished, an electromotive force will act round the circuit the total amount of which will be equal to the decrement of the number of lines of magnetic force in unit time.

III. When a dielectric is acted on by an electromotive force, it experiences what may be called electric polarization. If the direction of the electromotive force is called positive and if we suppose the dielectric bounded by the conductor is A on the negative and B on the positive side, then the surface of the conductor A is positively electrified and that of B negatively.

IV. When the electric displacement increases or diminishes, the effect is equivalent to that of an electric current in the positive or negative direction.

To him the aether was more real than matter. Stresses within it produced electric and magnetic forces. Vibrations were propagated across it as light. Speculation found scope in discerning how matter could pass through it, and in explaining such phenomena as stellar aberration. Arago cast doubt upon some of the properties ascribed to it. The "dragging coefficient" introduced by Fresnel did not work out quite as expected, and there was a general appeal for further experiments to account for the discrepancy.

Clerk Maxwell had not applied his theory to moving media. After him, therefore, Hertz took the equations and modified them to the required conditions. Fresnel's "dragging coefficient", however, still caused misgivings. Thereupon Lorentz introduced into the modified equations a fictitious variable called "the proper time", to take account of the motion of the

earth with respect to the aether. Einstein interpreted this variable, and thus brought Clerk Maxwell's equations into conformity with later theory. Concerning the accuracy and the interpretation of the tests upon which the validity of this later theory depend, judgment may properly be suspended, but concerning the basic soundness of Clerk Maxwell's equations there is common agreement.

The cause of the stability of Clerk Maxwell's work, here indicated, may be traced to the care he took to extend his results always to the most general case he could imagine. He taught us to conceive the energy of a material system as determined by the configuration and motion of that system, and to generalize our ideas of configuration, motion, and force "to the utmost extent warranted by physical conditions". He advised us "to become acquainted with these fundamental ideas, to examine them under all their aspects, and habitually to guide the current of thought along the channels of strict dynamical reasoning". Many have found the task difficult—so much easier is it to learn from a particular case than from one more general. It is for this reason that certain parts of Clerk Maxwell's writings constitute such hard reading for students at an elementary stage, and such delightful reading for investigators to return to when they have schooled themselves in mathematics and dynamics. It is for this reason also that the physicists of to-day, in search of inspiration, resort to the writings of Clerk Maxwell many times more often than to the writings of his contemporaries in the domain of natural science.

Our heritage from Clerk Maxwell is rich in ideas comprising notions of space and time, matter and motion, aether and light, electricity and magnetism. Of his own equipment, apparatus, and instruments, there are comparatively few relics. Thanks to Sir Ernest Rutherford, however, it has been possible to examine some of these treasures of the Cavendish Laboratory and to illustrate them.

Here let it be observed that Maxwell was of middle height, and strong of frame. He was possessed of dark eyes, jet black hair and beard, and in complexion he was somewhat pale. His mirth was real, but never boisterous—he was never fretful,

never irascible. In disposition he was genial and patient, and he had great power of concentration even amidst distractions. He had considerable knowledge and discrimination in literature, he was a rapid reader, and he had a retentive memory. He loved his dog, his horse, his friends—such are the characteristics and such the virtues recorded of him.

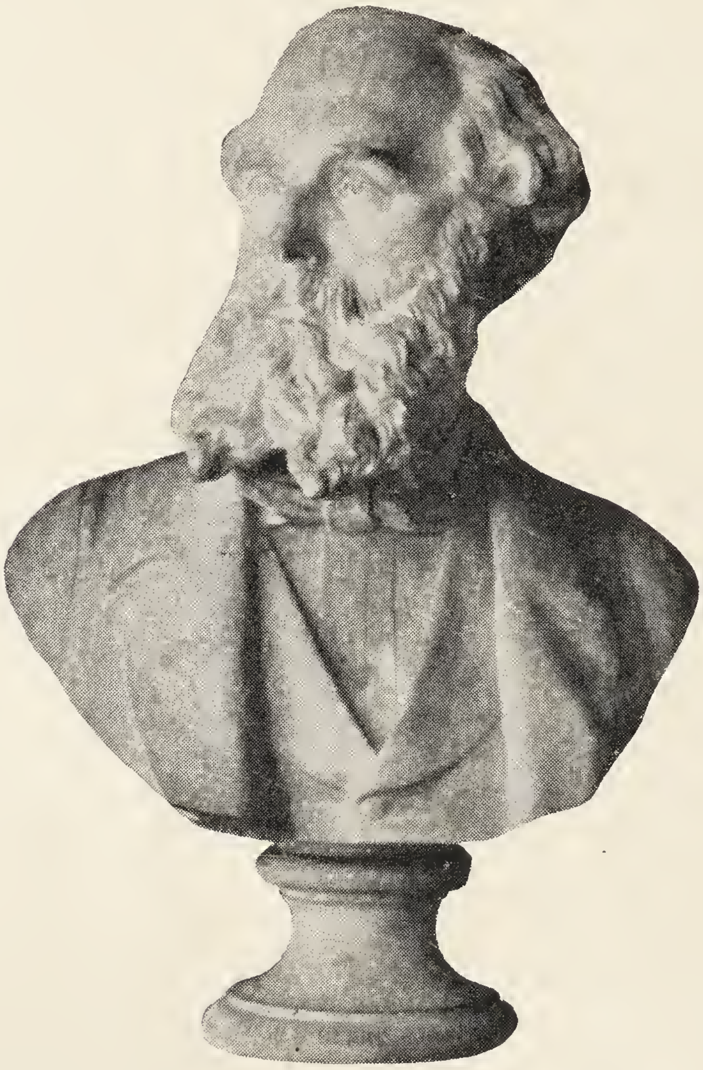


FIG. 1. JAMES CLERK MAXWELL. Marble Bust by Boehm.

The portrait in the frontispiece is copied from a photograph now on the staircase of the Cavendish Laboratory. At the Cavendish Laboratory also is the marble bust (Fig. 1) by Sir J. E. Boehm, R.A. This bears on the reverse side the inscription:

"$\frac{dp}{dt}$

Boehm Fecit

1879."

The cryptonym $\frac{dp}{dt}$ was occasionally used by Maxwell as a sub-

stitute for his initials "J. C. M." Writing J for Joule's Equivalent, C for Carnot's Function, and M for the rate at which heat must be supplied per unit increase of volume at constant tem-

Fig. 1a. J. C. Maxwell.

perature, $\frac{dp}{dt} = JCM$ becomes a statement of the Second Law of Thermodynamics. Fig. 2 is an example of the handwriting of Clerk Maxwell. The lower pages are from examination-questions

set by him in 1858. The upper page is from his notes on Cavendish and was probably written about the year 1870.

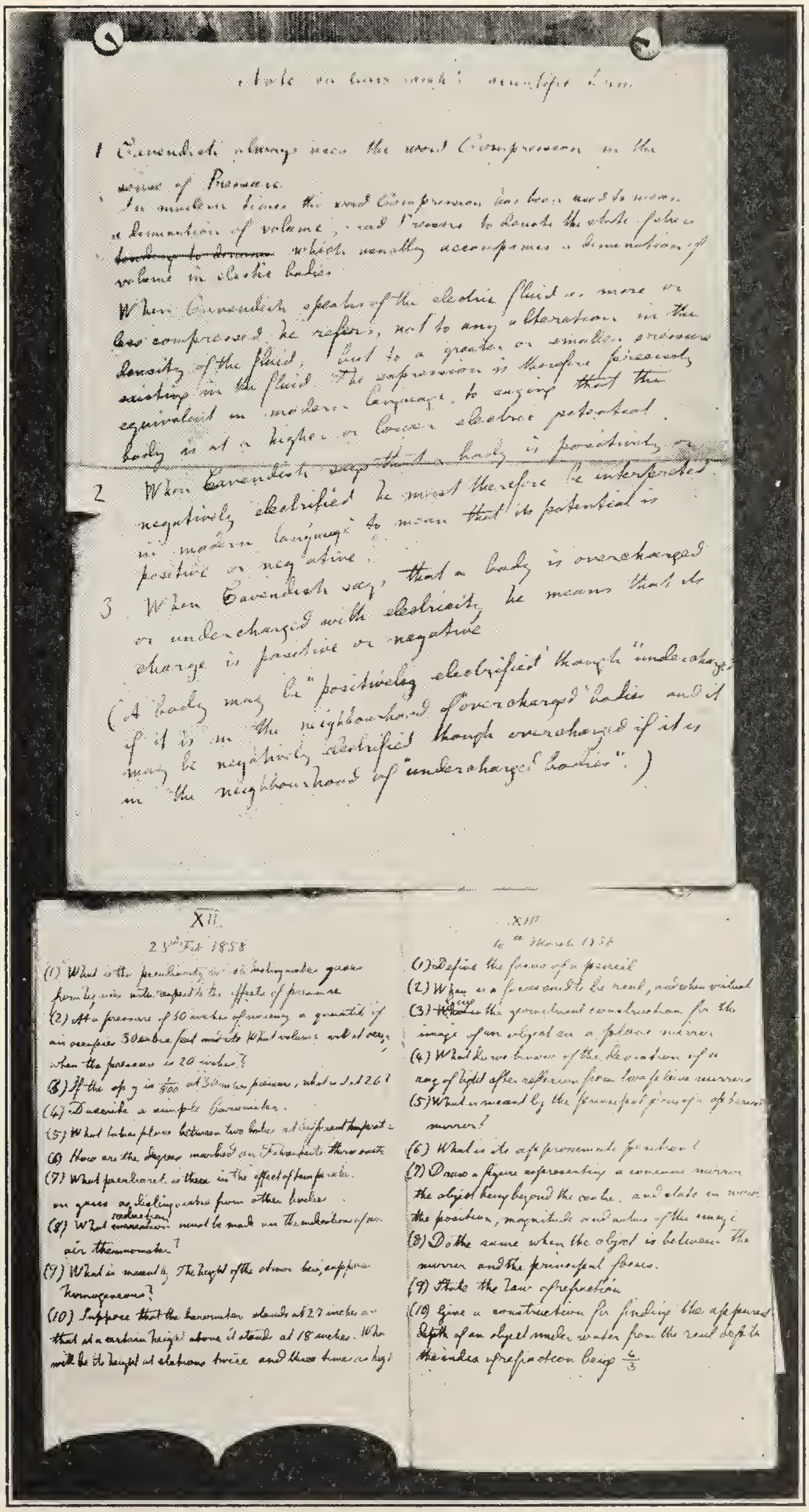


FIG. 2. HANDWRITING OF CLERK MAXWELL.

In 1857 the examiners for the Adams Prize Essay chose for their subject "The Stability of Saturn's Rings". It could be supposed that the rings were rigid, or fluid and in part aeriform, or that they consisted of masses not materially coherent. Maxwell worked very hard at this problem. He found that the only system that could exist is one composed of an indefinite number of unconnected particles—"a flight of brick-bats" as he afterwards termed them. His essay secured the prize, and at

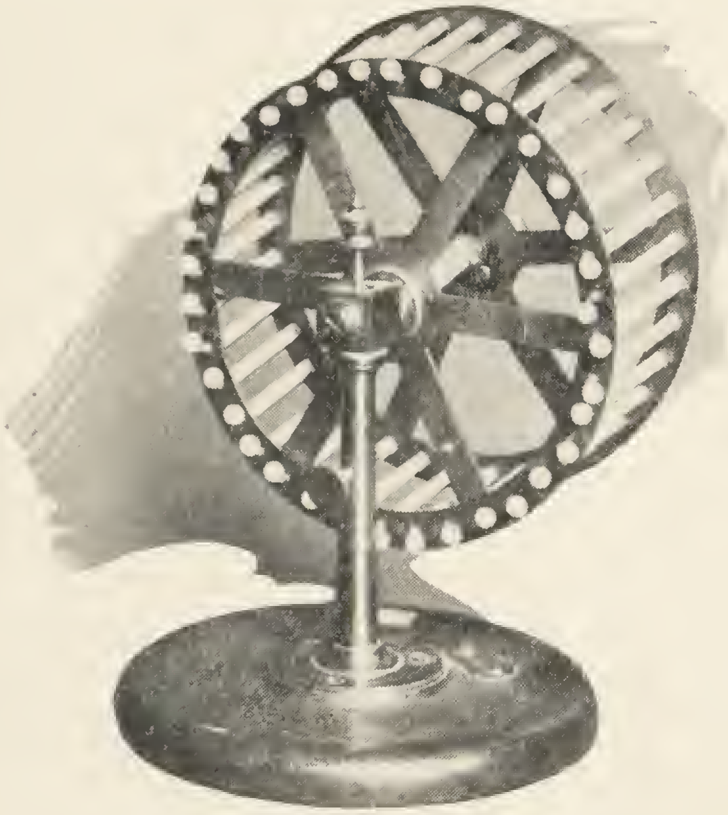


Fig. 3. Clerk Maxwell's Model of Saturn's Rings.

once placed him in the first rank of men of science. To exhibit the movements of the satellites he designed a model (Fig. 3) (*Scientific Papers*, vol. 1, pp. 286-376). He described this model to a friend as "two wheels turning on parallel parts of a cranked axle; thirty-six little cranks of same length between corresponding points of the circumference; each carries a little ivory satellite"; these satellites are made to go through the motions belonging to a series of waves. This description corresponds to the model in the Cavendish Laboratory. The ring can either be rotated as a whole about the central axis, or the central axis can be locked by inserting a pin, in which case the back brass circle rotates in its own plane about an axis eccentric to the

front brass circle, and simultaneously the white beads rotate in circular paths of small radius.

In April, 1857, Maxwell communicated to the Royal Society of Edinburgh (vol. xxi. part IV.) an account of a dynamical top (Fig. 4) for exhibiting the phenomena of the motion of a system of invariable form about a fixed point, with some suggestions regarding the Earth's motion. The paper illustrates to perfection the method of "proceeding from one distinct idea to another,

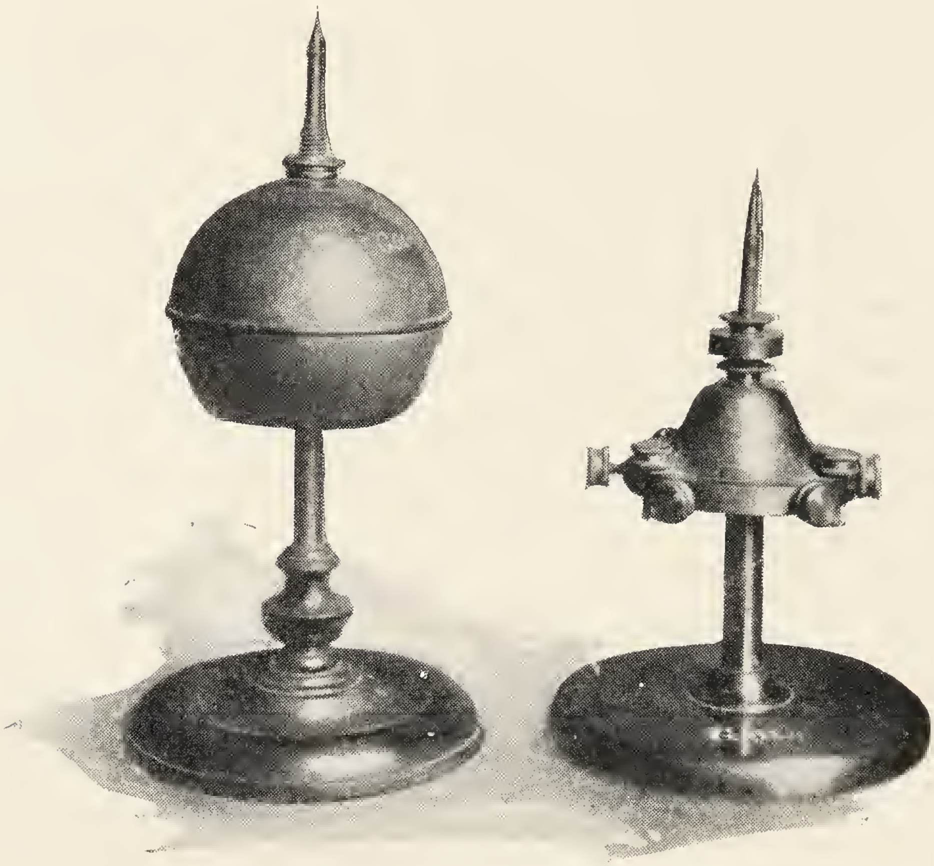


FIG. 4. CLERK MAXWELL'S DYNAMICAL TOPS. The larger top is of wood, the smaller is of brass.

instead of trusting to symbols and equations". It also develops the use of the method depending upon the compounding of angular moments. Provision was made for eleven adjustments of the dynamical top. The instantaneous axis about which the top is revolving is ascertained by means of a colour-disc placed near the upper end of the axis. Fig. 5 represents Maxwell's Spinning Wheel—"diabolo" pattern. The brass top (Fig. 4) was made by Ramage of Aberdeen, and Maxwell took it up to Cambridge with him in 1857 and exhibited it at a tea-party. His friends left it spinning, and next morning Maxwell, noticing one of them coming across the court, leapt out of bed, started

the top, and retired between the sheets. It is said that thereafter the spinning-power of the top commanded great respect as to its power of illustrating Poinsot's theory of rotating bodies. Ramage made such dynamical tops "for several seats of learning".

As a boy, one of Maxwell's amusements when not swimming, fishing, or riding, was making magic discs. In January, 1855, he described his theory of colours in relation to colour-blindness

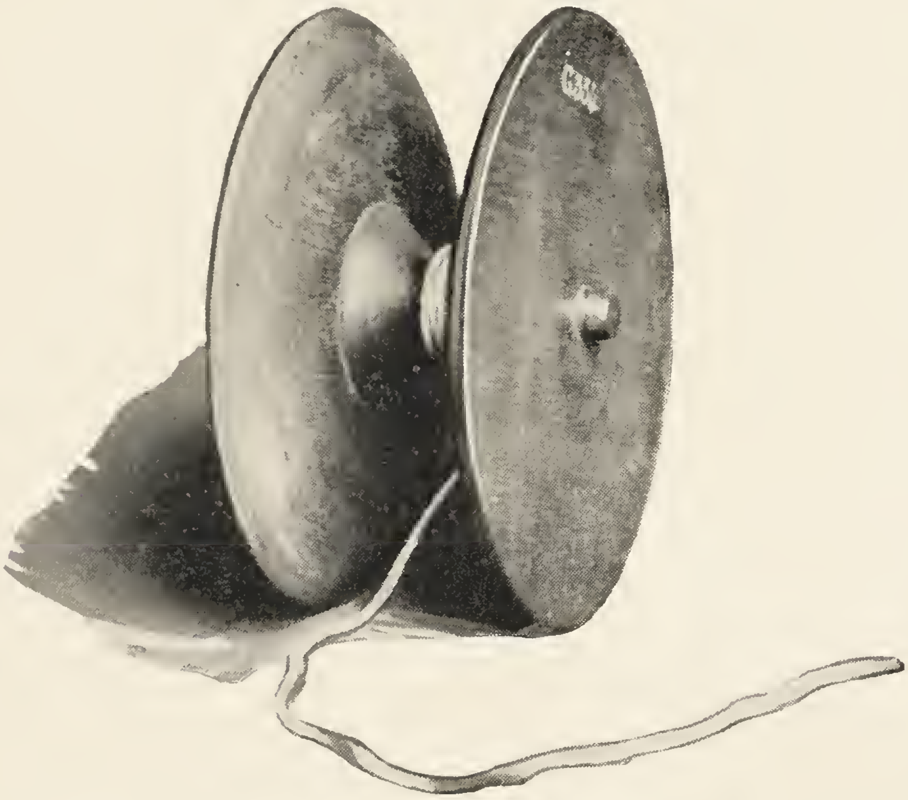


FIG. 5. CLERK MAXWELL'S "SPINNING WHEEL". Diabolo Pattern.

to the Royal Scottish Society of Arts, and explained the use of colour-discs for such investigations. By spinning the colour top (Fig. 6), carrying discs of various colours, each colour is presented to the eye for a time proportional to the angle of the sector exposed. Any given colour may be imitated by combining discs of different colours, and of different angular widths. Radial slits enable the angular adjustments to be made, and the proportions of the respective colours can be registered by the graduations on the rim of the top. By this means Maxwell derived his colour equations. In the outer ring the three sectors were vermilion, ultramarine, and emerald green, respectively; in the inner ring were two sectors—black and white. Writing

to his father about this top in 1855 he said, "I have a new trick of stretching the string horizontally above the top, so as to touch the upper part of the axis. The motion of the axis sets the string a-vibrating in the same time with the revolutions of the top, and the colours are seen in the haze produced by the vibrations."

In relation to these experiments on colour and vision

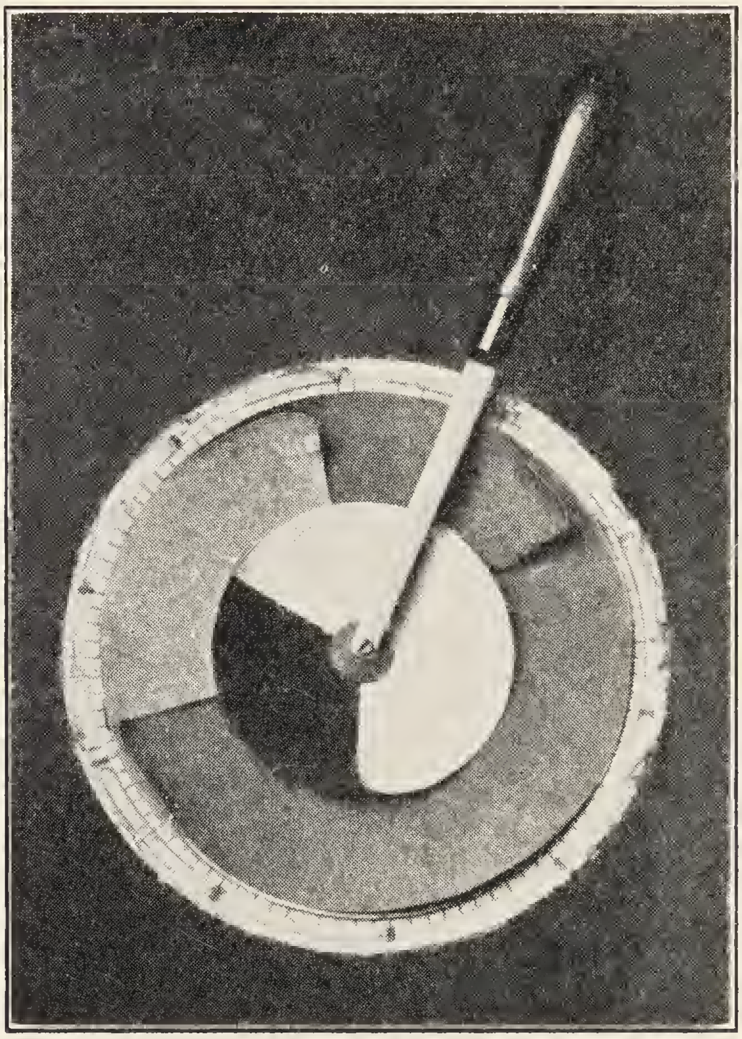


Fig. 6. Clerk Maxwell's "Colour Top". At the back, the spindle carries a grooved pulley for spinning by a string-drive. Also the edge of the pulley is milled for spinning by hand.

Maxwell said: "We are indebted to Newton for the original design, to Young for the means of working it out, to Professor Forbes for a scientific history of its application to practice, to Helmholtz for a rigorous examination of the facts, and to Professor Grassman (*Philosophical Magazine,* 1852) for an admirable exposition of the subject".

His paper on the theory of compound colours, and the relations of the colours of the spectrum (*Philosophical Transac-*

tions of the Royal Society, March 1860) describes two methods that involve what might be termed a "colour box". The apparatus (Fig. 11) at the Cavendish Laboratory may be identified as similar to that represented on Plate VII. of that paper; it is described on page 437 of *Scientific Papers*. It is provided with a light-proof lid.

Referring to the arrangement indicated in Fig. 7, light from a sheet of paper illuminated by sunlight admitted at the slits X, Y, and Z, falls upon the prisms P P′ (angles = 45°), and then on to a concave silvered glass S of radius 34 inches. After

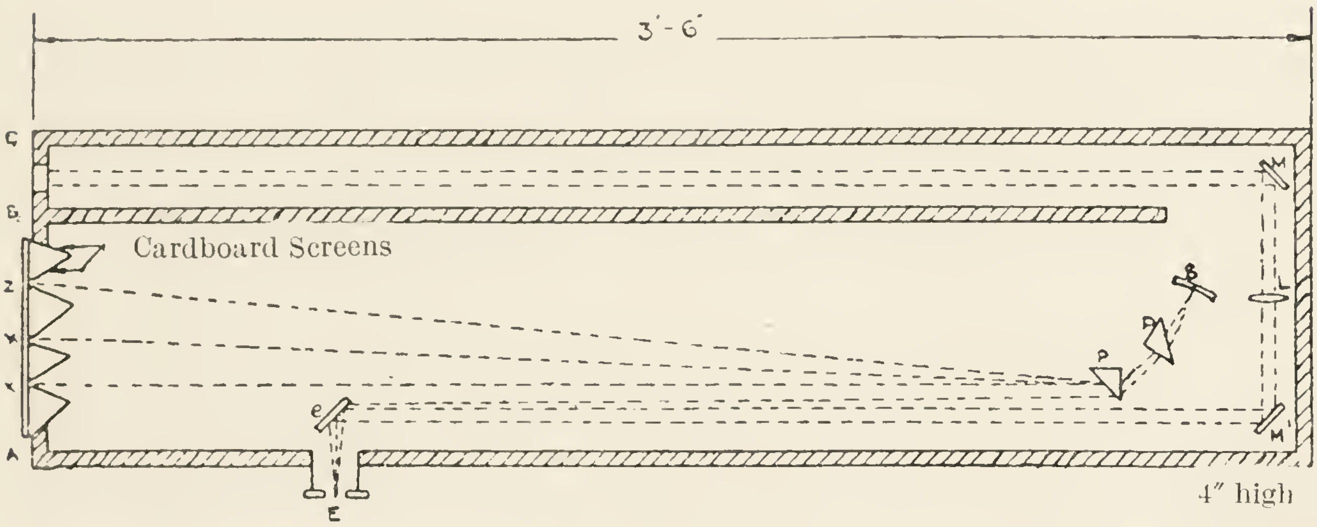


Fig. 7. Diagram of Clerk Maxwell's Colour Box. Length of Spectrum from Spectral Line A to Spectral Line R was 3·6″.

reflection, the light passes again through the prisms P′, P, and is reflected by a small mirror *e* to the slit E, where the eye is placed to receive the light compounded of the colours corresponding to the positions and breadths of the slits X, Y, and Z. At the same time, another portion of the light from the illuminated paper enters at BC, is reflected at the mirror M, passes close to the prism P, and is reflected along with the coloured light at *e*, to the eye-slit at E. In this way the compound colour is compared with a constant white light in optical juxtaposition with it. The mirror M is made of silvered glass, that at M′ is of glass "roughened and blackened" at the back, to reduce the intensity of the constant light to a convenient value.

Amongst his neighbours, his optical investigations caused some consternation. His house in London was at 8 Palace Gardens Terrace. It is now No. 16, and it is marked by the

London County Council with an appropriate plaque. He experimented at the window with a colour-box which was painted black, and nearly eight feet long. The people of Kensington decided that it was his coffin, and that his particular mental defect disposed him constantly to stare into it. It was at this house also that he experimented on the viscosity of gases at different pressures and temperatures (Fig. 10).

Maxwell made frequent use of the "moving-picture" of his day. The Wheel of Life was invented under the name of the Daedaleum by Dr. Horner of Bristol in 1838. In 1860 a patent

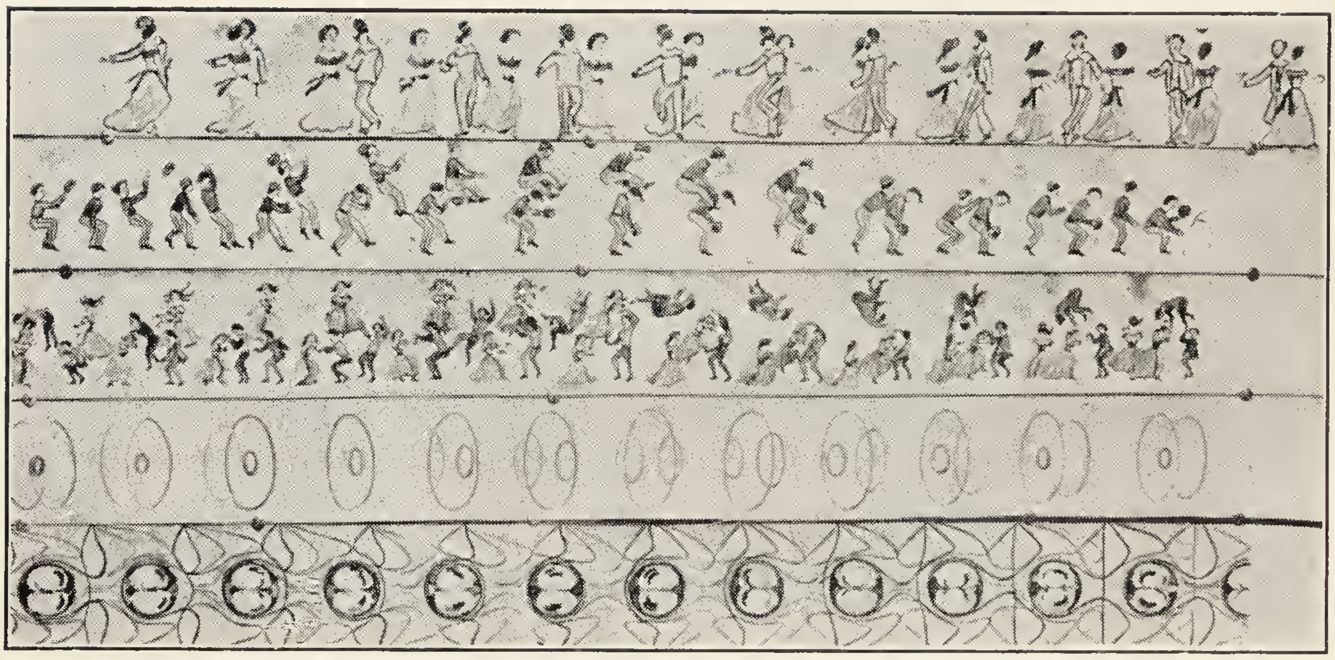


Fig. 8. Clerk Maxwell's Zoetrope Diagrams.

was taken out for the same apparatus by Devignes who called it a Zoetrope. A disc-form of a similar apparatus called a Phenakistoscope was invented by Plateau of Ghent, upon the suggestion of Roget (*Phil. Trans. R.S.*, 1825). This was produced under the name of the Fantoscope in 1833. The drawings in Figs. 8 and 9 are by Maxwell. The more serious ones represent respectively vortex rings passing through one another and expanding, and the movement of a conductor through the aether. The less serious are self-explanatory.

Maxwell's apparatus for determining the viscosity of gases is shown in Fig. 10. A system of circular discs is suspended by a steel torsion wire, coaxially with a fixed system, so that alternate discs are fixed and free to turn, respectively.

Fig. 11 illustrates the apparatus constructed for Maxwell in 1861 for the investigation of the kinetic energy of an electric circuit in rapid motion. The central electromagnet is capable of rotating about the horizontal axis between the pivots, within a ring which revolves about a vertical axis. The earth's field is neutralized independently. Current is led into the coil through the pivots. Observation is made to determine whether there is

FIG. 9. ZOETROPE USED BY MAXWELL.

any angular movement of the coil with respect to the vertical during the rotation of the ring. He deduced that if a magnet contains matter in rapid rotation, the angular momentum of this rotation must be very small compared with any quantities which he could measure. (*Elec. and Mag.*, vol. ii., pp. 211-22).

Fig. 12 is from a photograph of the Lecture Room designed and used by Maxwell, at the Cavendish Laboratory. Special features are the wide space for experiments behind the lecture-table, sliding panels in the roof giving access to beams for

suspensions, and sliding side shutters for darkening—operated from floor level.

The illustration in Fig. 13 of Maxwell's model to demonstrate the equations of electric currents, especially for the case of two inductive circuits, is from a photograph of the apparatus at King's College, taken by kind permission of the authorities. A description of this model is to be found in Andrew Gray's *Treatise on Magnetism and Electricity*, pp. 344-45. There is a similar model at the Cavendish Laboratory.

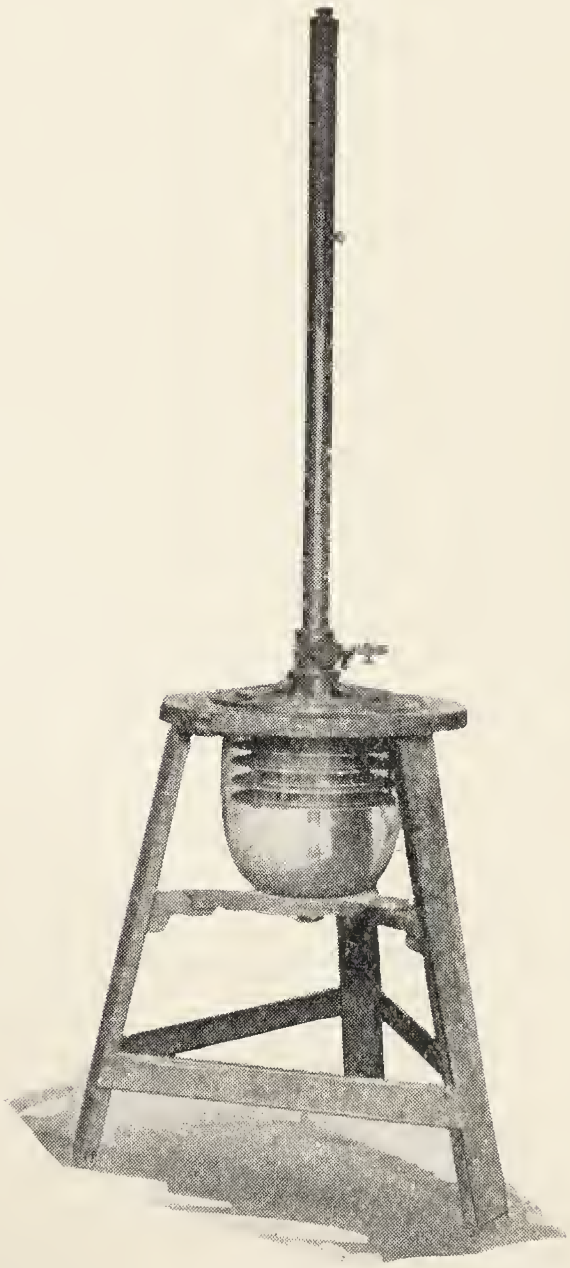


Fig. 10. Maxwell's Apparatus for Determining the Viscosity of Gases.

On behalf of a Committee of graduate members of the University of Cambridge, and of other friends who were desirous of securing a fitting memorial to Clerk Maxwell, the late W. D. Niven, F.R.S., undertook the work of editing the *Scientific Papers*, and of reproducing them in two volumes. These were published by the Cambridge University Press in 1890.

The first complete book written by Clerk Maxwell was the *Theory of Heat* (1871). It was followed by his work on *Electricity and Magnetism* (1873) in two volumes. In 1876 there appeared his treatise, small but of inmeasurable worth, entitled *Matter and Motion*. Finally, in 1879, there came from the Cambridge University Press his account of *The Electrical Researches of the Honourable Henry Cavendish*, F.R.S. The intention of Clerk Maxwell had been also to write for students an introductory text-book on the theory of electricity, and he prepared the

manuscript of the greater part of it. This was subsequently completed by Professor Garnett, and was published in 1881 with the title *Elementary Treatise on Electricity*.

Although in this literature the records of Clerk Maxwell's scientific contributions are well preserved, the account of his life has yet to be treated adequately. *The Life of James Clerk Maxwell* (1884), by Lewis Campbell and William Garnett, is a faithful compilation of selections from his correspondence, from

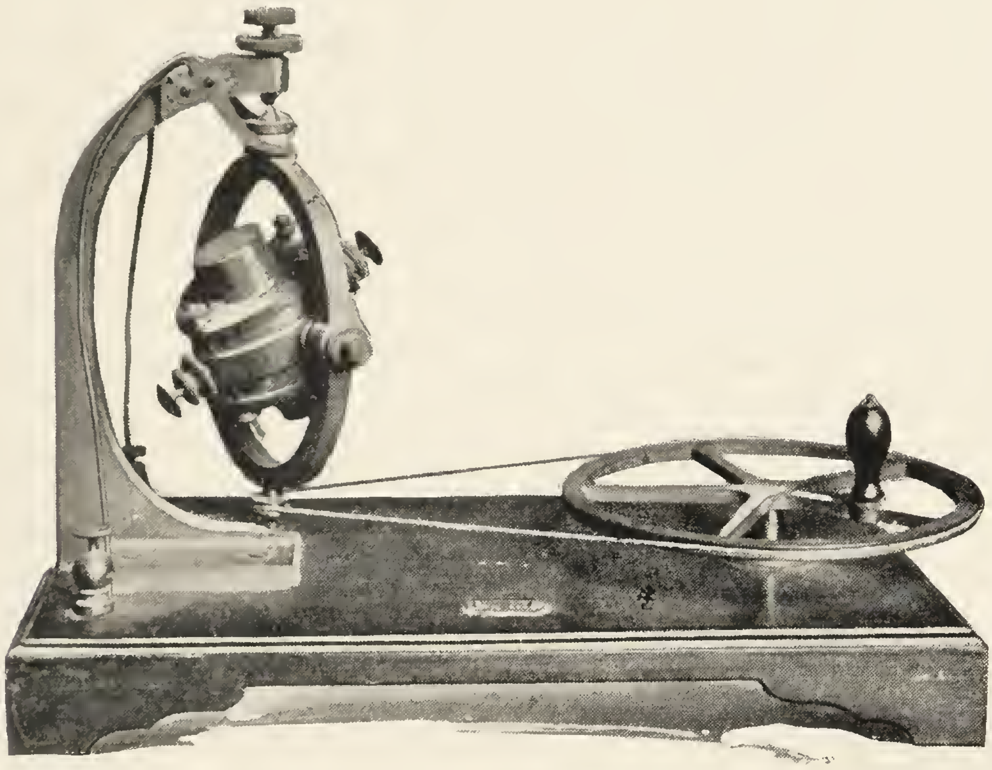


Fig. 11. Maxwell's Apparatus for Investigation of Possible Inertia of Electricity.

his occasional writings, and from his numerous experiments in versification. It also contains particulars of his career; but it leaves to futurity the task of revealing yet more of the man and his work, and of demonstrating his leadership in contemporary thought, and his influence upon subsequent progress. His close and happy companionship with Kelvin and Tait may furnish the clue needed by such a biographer; for in such treatises as Tait's *Thermodynamics* and C. C. Knott's *Life and Scientific Work of Tait*, Clerk Maxwell is frequently in evidence.

On the occasion of his Rede Lecture at Cambridge in 1878, which was his last public utterance, James Clerk Maxwell asked his audience to regard the telephone as a material symbol of

the widely-separated departments of human knowledge, the cultivation of which led by as many converging paths to the

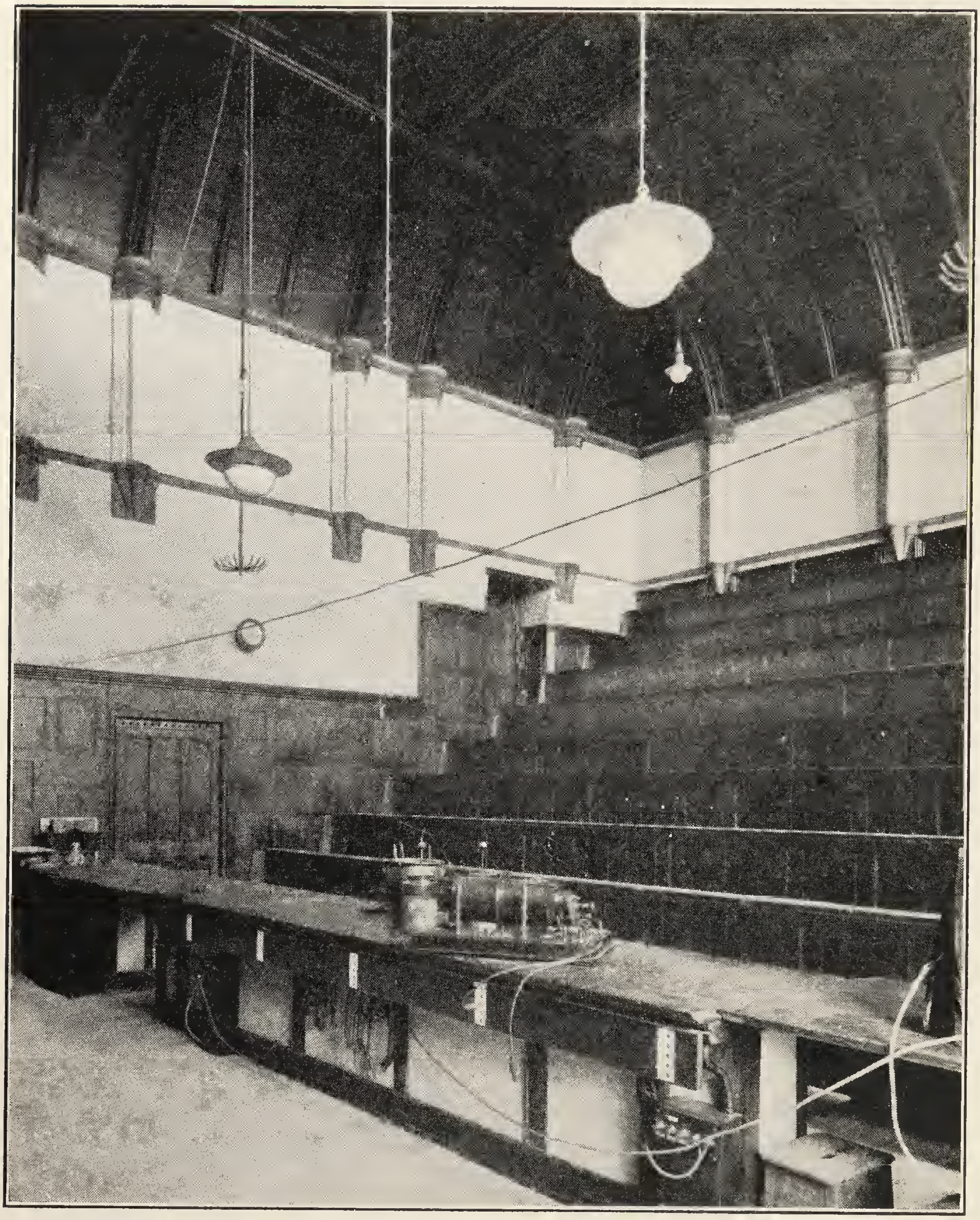


Fig. 12. Clerk Maxwell's Lecture Room at the Cavendish Laboratory, Cambridge.

invention of that instrument by Professor Graham Bell. From his youth up, through the wilderness of these departments

Maxwell had wandered and had realized the extent to which knowledge concerning physical science was advancing in the Victorian age. That natural phenomena were the result of forces acting between one body and another had for centuries been conceived, but his task broadly was to direct attention to the distribution and balance of energy as determined by the configuration and motion of a material system. Henceforward progress was to be along the channels of strict dynamical reasoning, aided by the science of experimenting accurately. He declared it to be "the glory of true science that all legitimate methods must lead to the same final results". He took heed lest the multiplication of symbols might put a stop to the development. Accordingly he directed his efforts to "sweeping cobwebs off the sky".

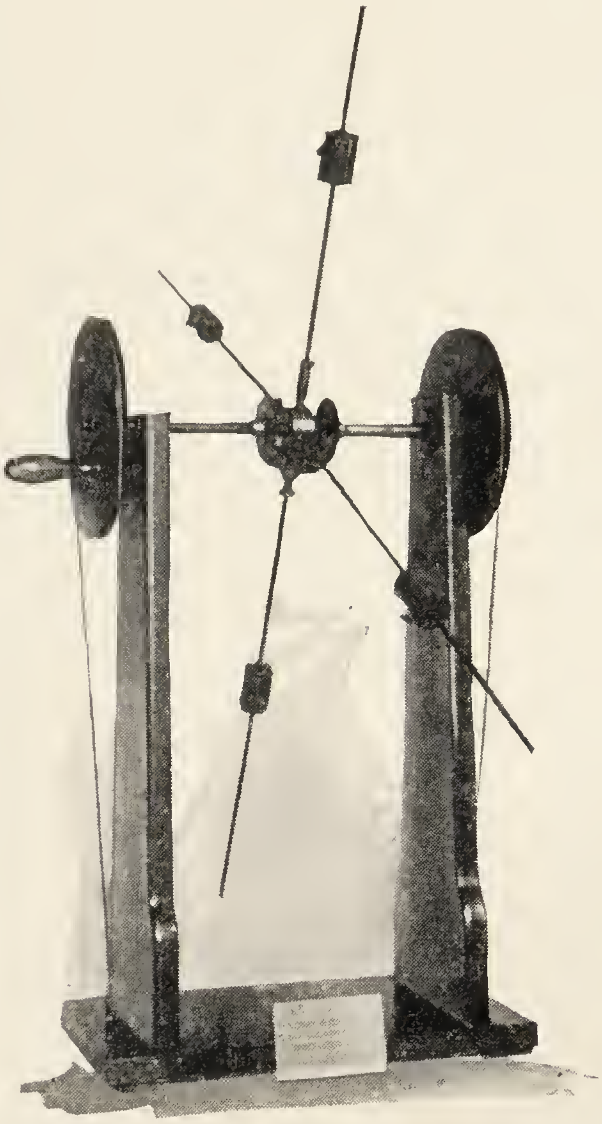


Fig. 13. Maxwell's Dynamical Model to Illustrate the Equations of Electric Currents.

It was no mean realm of learning into which Clerk Maxwell entered at Cambridge. Newton (1642–1727) in 1669 had there succeeded to the Lucasian chair vacated by Barrow. Roger Cotes (1682–1716), though "his style was concise even to obscurity", was a Cambridge mathematician of a high order. Next in the line was Robert Smith (1689–1768), the founder of "Smith's Prize", the Master of Trinity, and the Master of Mechanics to George II. Then followed the famous George Atwood (1746–1807). William Whewell (1794–1866), the son of a carpenter of Lancaster, was also the Master of Trinity; he

raised the standard of Cambridge education, awakened interest in natural philosophy, helped to found (1818) the Cambridge Philosophical Society, and produced works that considerably relieved the dull mechanic exercises of the pure analysts. Maxwell's object, and that of the joyous fraternity to which he belonged, was to extend the later teaching. Briefly, he interpreted Physical Science as the cultivation of the sense of energy, and as the guidance of thought along the channels of dynamical reasoning.

As an example of his "occasional verse", the following bears appropriately upon electrical communication and indicates his substantial belief in the future of telegraphy. It was written by him in 1857 while on a railway journey to Glasgow.

THE SONG OF THE ATLANTIC TELEGRAPH COMPANY

Let (U) = "Under the Sea".

I.

2 (U)

Mark how the telegraph motions to me,

2 (U)

Signals are coming along,
With a wag, wag, wag;
The telegraph needle is vibrating free,
And every vibration is telling me,
How they drag, drag, drag,
The telegraph cable along.

II.

2 (U)

No little signals are coming to me,

2 (U)

Something has surely gone wrong,
And it's broke, broke, broke;
What is the cause of it does not transpire,
But something has broken the telegraph wire,
With a stroke,
Or else they've been pulling too strong.

III.

2 (U)

Fishes are whispering. What can it be,

(2 U)

So many hundred miles long?
For it's strange, strange, strange,
How they could spin out such durable stuff,
Lying all wiry, elastic and tough,
Without change, change, change,
In the salt water so strong.

IV.

2 (U)

There let us leave it for fishes to see;

2 (U)

They'll see lots of cables ere long,
For we'll twine, twine, twine,
And spin a new cable, and try it again,
And settle our bargains of cotton and grain,
With a line, line, line—
A line that will never go wrong.

In 1866 Maxwell returned to Cambridge as Moderator in the Mathematical Tripos. There was a movement in favour of introducing problems relating to heat, electricity, and magnetism into those examinations, and Maxwell was instrumental in bringing about the reforms. In 1870 the Duke of Devonshire expressed a desire to build and to furnish a Physical Laboratory for Cambridge. A professorship was thus rendered necessary, and accordingly the Senate founded in 1871 the chair of Experimental Physics. To this Maxwell was appointed on March 8 of that year. He devoted himself whole-heartedly to the task of designing and superintending the erection of the now world-famous Cavendish Laboratory. By 1873 he had completed his book on electricity and magnetism and it had been published, and he had begun the labour of going through the electrical researches of the Hon. Henry Cavendish (1731–1810),

the first of the quantitative electricians. The Duke of Devonshire had supplied the means of equipping the laboratory, and he generously proposed to present any additional apparatus that was needed for the advancement of science. Maxwell lectured there on Heat and the Constitution of Bodies, Electricity, and Electro-Magnetism—his inaugural lecture in October, 1871, was on Colour Vision.

The work at "the Cavendish" thus began under the most favourable conditions. Maxwell encouraged and was encouraged by the new devotees of physics. He derived, for example, special satisfaction from the success achieved by George Chrystal in the verification there of Ohm's law for metallic conductors; and although some have lamented the deviation of his own line of research at this period to the records of Henry Cavendish, there is no doubt that he found in those records something inspiring—possibly it was their close bearing upon the relation between physical phenomena and sensation.

Although half a century has now passed, it is still hard to write that in November, 1879, at the prime of normal life, Clerk Maxwell, the supreme interpreter of the world of Physics, died. To those proceeding to extend his victories along the road of electrical communication he left his sword and his chariot—his equations and his theories. He left also an example of individual thought and achievement and a plea for fellowship between all men of science that proves his cherished motive and purpose to have been no less exalted than his consummate mind.

Portrait of Ampère. From a Lithograph of a Drawing from Life. By Boilly.

II

ANDRÉ MARIE AMPÈRE

France, amidst her greatest woes, has always produced men who could establish her glory. It was thus in keeping with immemorial precedent that in the troubled period of transition from the eighteenth to the nineteenth century there should be born, at Lyons, André Marie Ampère, a genius destined to found a new era. At the date of his birth—January 20, 1775—Lyons was the chief centre of French commerce, and France was entering upon a revolution. Louis XVI., with the best intentions but with deplorable ineptitude, had succeeded to the throne scarcely a year previously. The people of France had developed beyond its institutions, and neither the monarch nor his ministers possessed the qualities needed to sweep away abuses. Accordingly it happened that the boyhood and youth of Ampère were passed at the seething focus of the reaction that culminated in outbreaks against the combined forces of the Dantonists, the Robespierrists and the fatal Commune. Upon the industries of Lyons the consequences descended heavily, for 30,000 of its silk workers were thrown out of employment; and in the midst of the turmoil Ampère's father, on November 23, 1793, fell on the scaffold—a victim of the *terreur*.

André was then eighteen. The shock broke the delicate threads that sustained the balance of his mind. He passed his days in silence, listlessly contemplating the sky, or aimlessly making with his fingers pyramids of sand. In vain his friends tried to arouse him. All faculty, all sentiment, was for the time obliterated. The blow was intensified because of the loss of the companionship that had existed between him and his father. His father had been his only instructor. Finding that the natural

bent of his son was mathematics, he had permitted him to discover his own path, with the result that by the age of eleven, André had acquired sufficient facility to apply algebra and geometry to his problems. When he was twelve his father had introduced him to the Librarian of the College of Lyons, who showed him copies of the works of Euler and Bernoulli. This had given an impulse to his studies, and he had devoted himself more closely to Latin in order to be able to learn about the calculus. It is recorded that just before his father's death André read the *Mécanique analytique*, of Lagrange, and worked through almost all the calculations it contains. His memory even then was prodigious, and his perceptive faculties were astonishing.

For a year after the tragedy, the cloud overshadowed him. Then it began to lift. His interest was aroused first by glancing at a book by Rousseau on botany; thus he was led again to prose, and from prose to verse. Measured language numbed the mental pain and quickened his intellect. His own attempts at poetry soon became interspersed with x's and y's and there was produced a formula for determining all the powers of a polynomial. It was poetry lacking the art of versification, but it was blazoned with algebra in amazing variety. His brain was again trying to work, and the effort only needed direction.

Direction came in the usual way. His mother had a little property at Polémieux-les-Mont-d'Or—about nine miles north of Lyons. It was there that André's boyhood had been spent with his parents. The house—"la petite maison blanche"—and garden exist there to this day (Figs. 1 and 2), resting peacefully upon the hillside, unchanged—as if still celebrating his obsequies. You may approach Polémieux by road from Lyons by way of Mouton and Limonest, and then by a pleasant descent through about eight kilometres of country lanes to the village in the hollow. You may best return to Lyons by the road that wanders near the stream, to Neuville, and thence by the track that borders the Saône. It was in this picturesque district of Mont-d'Or that Ampère found direction. Amongst his notes of that melancholy period there is at last an entry:

> Un jour que je me promenais après le coucher du soleil le long d'un ruisseau solitaire. . . .

The memorandum there ends, but it suffices to record his first

Fig. 1. Polémieux, Showing the monument to Ampère. The house in which the Ampères lived is just beyond the last bend of the road on the left of the statue.

Fig. 2. Ampère's House at Polémieux.

meeting with Julie Carron, and the first phase of a romance that is inseparable from his story.

They were married on August 6, 1799, and the register supplies particulars of their parentage: "André Marie Ampère, youngest son of J. J. Ampère, deceased, and of Antoinette de Suttières Sarcey, of the parish of Polémieux, Mont-d'Or, to Catherine Julie Carron, eldest daughter of the late Claude Carron and of Antoinette Boyron of the parish of Saint Germain, Mont-d'Or."

Their combined income was meagre. They went to live at Lyons, but as he could not then (1800) obtain there sufficient recompense, he accepted a professorship at the École Centrale du Département de l'Ain which had been established at Bourg-en-Bresse. As this institution is so closely associated with his epoch-making researches, its history deserves more than passing notice.

There was a communal school at Bourg as long ago as 1391, presided over by a lay-rector. For centuries it was situated in La Verchère. In 1404 the school in common with everything else suffered as the result of plagues and wars. There was a development in 1561, when poor children were admitted free; and in 1572 it was enlarged and the lay personnel was increased. Ecclesiastical troubles followed, which brought the Jesuits to Bourg in 1614. A chair of philosophy was founded there in 1661, and it became in 1744 a centre of intellectual life, inspired by the astronomer Lalande. In 1751 the college was rebuilt, and in 1761 a physical laboratory was set up, and a second course of philosophy was introduced. Two years later the Jesuits were suppressed, and those at Bourg quitted the college. They were replaced by seven priests all born in the town. In 1793 the college was closed, but it was reopened under the name of the École Centrale. It was there that Ampère was a professor from 1801 to 1803. In 1805 the École Centrale was replaced by a municipal secondary school (Fig. 3).

The distance from Lyons to Bourg at the beginning of the nineteenth century was too great to permit Ampère to return each day to his home. Circumstances obliged him to live in lodgings in Bourg, while his wife remained in Lyons; for on August 12, 1800, their son Jean-Jacques was born. Ampère's lofty ideals at this period are in such strange contrast to his

surroundings that when the high quality of his scientific achievements at Bourg is kept in mind a profound lesson is taught of devotion in uncongenial environment. His aspirations may be gathered from a single paragraph of a letter he wrote at this time to his wife:

Quelle gloire attend celui qui mettra la dernière pierre à l'édifice de la physique moderne; quelle utilité ne doivent pas en espérer les arts les plus nécessaires à l'humanité.

FIG. 3. THE LYCÉE LALANDE AT BOURG-EN-BRESSE.

Those luminous thoughts, however, must be sought amidst the record of grim facts:

Je suis en pension, à quarante francs par mois, chez Beauregard; on me demandait soixante francs à l'auberge de Renaud, où il fallait manger avec les plus grands sottisiers que j'aie vus de ma vie. Cela passait toute expression. . . . J'ai vu le cabinet de physique, le laboratoire de chimie et l'unique petite *chambre* avec alcôve, petit débarras à mettre le bois. J'ai été fort content des machines; le laboratoire a un grand manteau de cheminée par où doivent s'exhaler les vapeurs nuisibles; il y a assez de ressources pour les différentes expériences . . . adresse tes lettres au citoyen Beauregard. . . .

and we find her addressing them accordingly:

De Mme. Julie Ampère au citoyen Ampère. Chez le citoyen Beauregard, professeur d'histoire à l'École Centrale du département de l'Ain, à Bourg près l'église Notre-Dame.

Further details of his daily routine followed with the confession that he had his supper at six, very tired after having

pilé, broyé, porté du charbon et soufflé le feu pendant douze ou treize heures, mais content d'avoir réussi quelquefois. . . .

Consolation, tinged with the mildest of admonishment, came with little delay from Lyons:

. . . prenons patience et réjouissons-nous de pouvoir parler à Pâques de tout ce que nous avons dans l'âme . . . mon pauvre Ampère, tu es trop content de m'envoyer tout ce que tu gagnes. . . . Tu fais donc toujours ces vilaines drogues.

This correspondence, which has been described as a veritable conjugation of the verb *aimer*, ends abruptly, for calamity once more overtook the great philosopher, and plunged him into despair. His Julie died on July 13, 1803.

The drama is intensified by the struggle that Ampère had made to obtain an appointment in Lyons whereby he hoped to secure means to alleviate the sufferings of his wife, and by the coincidence that Bonaparte, as first Consul of the Republic, had just nominated him, on the advice of Lalande and Delambre, as a professor at the Lycée of that city. The world was left to him desolate, and he naturally sought to withdraw from the scenes that once delighted and now tortured him.

In November, 1804, Ampère was nominated for an appointment at the École Polytechnique at Paris. Four years later he became Inspecteur Général of the University of Paris. In 1809 he was professor of Analytical Mathematics and Mechanics at the École Polytechnique, and he became a Chevalier de la Légion d'Honneur. From 1806 to 1810 he was a member of the Bureau Consultatif des Arts et Métiers, and in 1814 a member of the Institute. He entered the Académie des Sciences in the section of Geometry. During this period his activities were as intense, thorough, and illuminating as they were multifarious. For with mere erudition he was never content; he sought the

truth, elevated ideas, general principles, and he preferred them if they could be directly applied. A survey of his memoirs shows that his investigations relate to transcendental mathematics, applications to mechanics, electricity and magnetism, optics, the theory of gases, molecular physics, animal physiology, the theory of the earth, metaphysics and psychology for which he confessed he had a "passion". He was always a man of science, and in everything a man of fervour. Intermittently he was a man of faith.—"Le doute," he wrote, "est le plus grand des tourments que l'homme endure sur la terre."

The first memoir contributed by Ampère was published in Lyons in 1802. Following upon the work of Pascal, Fermat, and Buffon, it dealt with the mathematical theory of play, and with the evaluation, in accordance with the laws of probability, of the danger that awaits a player who takes a prescribed chance. This was presented to the Institute by Delambre, and it resulted in Ampère being appointed as professor at the Lycée de Lyon. Three years later he extended this investigation and published an account of his researches on the application of the Calculus of Variations to problems in mechanics. In 1806 he was at work at the theory of derived functions leading to a demonstration of Taylor's Theorem, and he was also giving a demonstration of the principle of virtual velocities. From that time until 1815 he contributed other papers on purely mathematical subjects; he proceeded into molecular physics and chemistry, and added a memoir upon the determination of the proportions in which bodies combine, taking account of the number and disposition respectively of the molecules of which the integral parts are composed.

His attraction to physics and chemistry at this period steadily increased, and he was writing on such subjects as Mariotte's Law, the classification of simple bodies, and the magnetic state of conductors. Then followed his brilliant series of memoirs on electro-dynamics, for which most of his previous studies was unconsciously a preparation. His immortal work on the mutual action of two currents was published in *Annales de Chimie et de Physique*, vol. xv., pp. 59-170, 1820. His last efforts were concentrated upon the classification of the sciences,

and upon determining the shape of the surface of luminous waves in a medium in which the elastic constants are different in the three dimensions of space.

It is rarely that a phenomenon in physics appears suddenly in the firmament of knowledge. Between the years 1802–1820 there were portents concerning the action of an electric current upon a magnetic needle, for Romognosi published on August 3, 1802, in the *Gazetta di Trento*, his paper entitled "Articulo sul Galvanismo". The great advance, however, began on July 21, 1820, when Hans Christian Oersted issued his four-page pamphlet *Experimenta circa effectum conflictus electrici in acum magneticam.* Information concerning the discovery reached Paris, by way of Switzerland, on September 11, 1820. An academician who had just returned from Geneva repeated before the Académie des Sciences the experiment of the Danish philosopher, and on September 18, Ampère presented his memoir and demonstration that two electric currents exert mechanical forces upon one another. His contributions to the subject were read at its séances on September 18 and 25, and October 9 and 30 of that year.

In the First Mémoire of this series of discourses, after reviewing all the facts observed by Oersted, Ampère distinguished between the directive action and the attractive or repulsive action. He introduced the astatic needle, and he showed that the directive force of a current is always exactly perpendicular to the direction of the current. He also carried out a demonstration with his "galvanoscope", and he announced that a conducting helix traversed by a current exerts an action resembling that of a bar magnet in all respects, and hence that the earth's magnetism could be explained by terrestrial electric currents circulating in the direction from east to west. Such currents, he suggested, might be caused by chemical action between the heterogeneous materials in contact within the globe, in accordance with the principle established by Volta for metals. He declared that whatever hypothesis might ultimately be adopted there would remain these three new facts: (i) Two electric currents (Fig. 4) attract one another when they are in the same *sens*, and they repel one another in the contrary case;

(ii) These attractions and repulsions are absolutely different from the (static) electric attractions and repulsions previously known; and (iii) A magnet acts in all circumstances, whether upon an electric current or upon another magnet, as if it consisted of an assemblage of closed electric circuits each traversed by a current. He emphasized the fact that in this electrodynamic action consideration has to be given rather to plane areas than to straight lines. He also drew attention to the effect of increasing the number of turns, and to the comparatively small importance of shape of area. Moreover he introduced the idea that elementary portions of circuits could be dealt with, as regards their electric currents, in accordance with the principle of the parallelogram of velocities, *i.e.*, that the action between two infinitely small portions is a function not merely of the

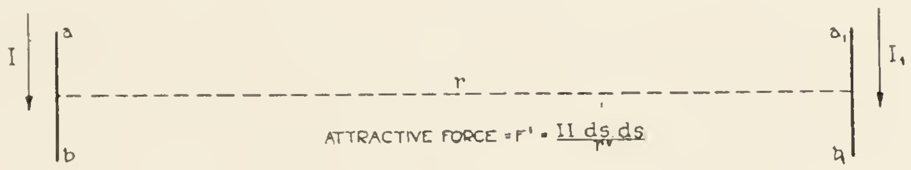


FIG. 4. PARALLEL ELEMENTS OF CIRCUITS.

distance, but also of the angles that determine their relative positions in space (Fig. 8).

His Second Mémoire, read at the Académie Royale des Sciences on June 10, 1822, relates to his determination of the formula that represents the mutual action of two portions, infinitely small, of "conducteurs voltaïques". The impossibility of subjecting infinitely small portions to experiment had been overcome by him by precise observations upon portions of finite length placed successively with respect to one another at different distances and in different positions—*i.e.* at different angular settings. He also adopted, with great advantage, null methods of measurement, and the method of counting oscillations.

For a concise account of his original researches in electrodynamics, attention may be directed to his treatise entitled *Exposé des nouvelles découvertes sur l'Électricité et le Magnétisme de MM. Oersted, Arago, Ampère, H. Davy, Biot, Erman, Schweiger, De la Rive, etc.*, which was published in 1822. The

principles enunciated by him have for a century been subjected to analysis and they have been recognized as the solid rock upon which the structure of electrical theory must be built. He discovered the mechanical action between electric currents, and he established mathematically and by physical demonstration the law of that action. This Maxwell declared to be one of the most brilliant achievements in science; for the whole, theory and experiment, had "leaped full grown and fully armed from the brain of the Newton of electricity", perfect in shape, unassailable in accuracy, and summed up in a

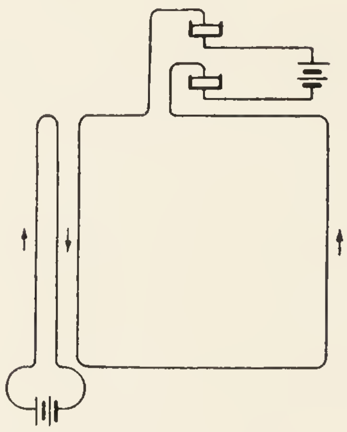


Fig. 5*a*. Straight Circuit Doubled on Itself.

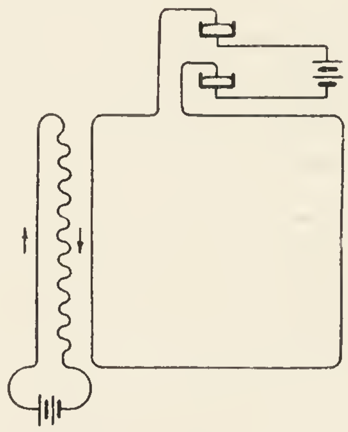


Fig. 5*b*. One Straight and One Sinuous Circuit Doubled.

formula from which all the phenomena could be deduced—the cardinal formula in electro-dynamics.

Ampère's theory begins with the reasonable assumption that the action between two very small elements of a circuit, or circuits, conveying an electric current is in the straight line joining those elements (Figs. 4-8), and that the effect is directly as the product of the steady currents in the two elements, respectively, and directly as the lengths of the elements. Then follow his experimental laws of steady currents so far as the mechanical force upon other conductors is concerned. He found that two equal currents close together (Fig. 5*a*) in opposite directions neutralize each other; if two wires close together—one straight and the other containing small sinuosities (Fig. 5*b*) but approximately straight—have the same current through them, their effects neutralize each other; if a conductor (Fig. 6)

whether carrying a current or not, is free to move only in the direction of its length, a current in a closed circuit of any kind placed near it is unable to move it; the action of a closed circuit

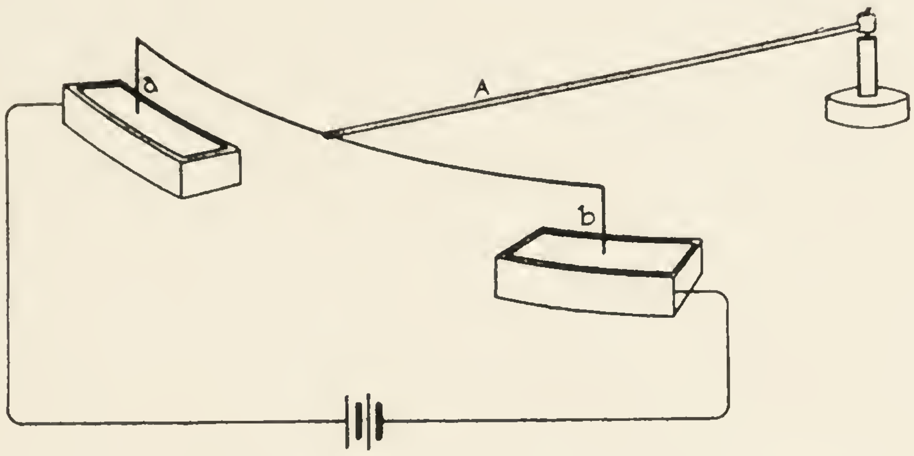


Fig. 6. Conductor Constrained Except Along the Direction of its Length.

on any portion of another circuit is perpendicular to the latter circuit. He showed that every linear conductor carrying a current is equivalent to a magnetic shell, the bounding edge of which coincides with the conductor, and that the moment per unit area of the shell—*i.e.* the strength of the shell—is pro-

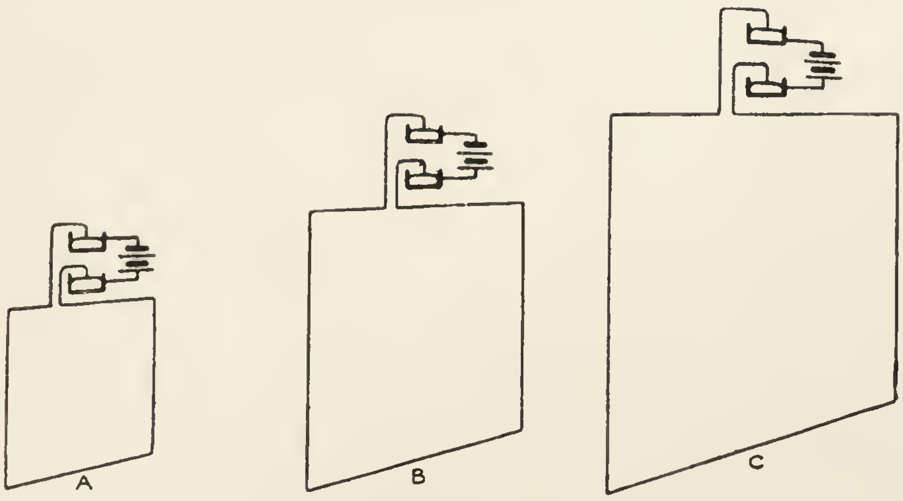


Fig. 7. Apparatus to Demonstrate Law of Force Between similar Circuits—Loops of different Sizes and Spacings.

portional to the current. He also proved by experiment that if there are three conductors A, B, C (Fig. 7), bent into similar plane closed figures, but of different dimensions—such that C is N times greater than B, while A is one-Nth of B—and placed at different distances from one another, with the same current

through each conductor, there is mechanical equilibrium, provided that the distance BC is N times greater than the distance AB.

De la Rive, who was in close touch with the time and a personal friend of Ampère, recalls the diversity of views held concerning the discovery of the mutual action between currents. It was the first example of a set of forces in which the actions are exerted in lines at right angles to the respective directions of the forces, and not in the direction of the forces themselves. Moreover, the intensity depends not simply upon the distance r between acting particles of conductors, but upon a variable element—

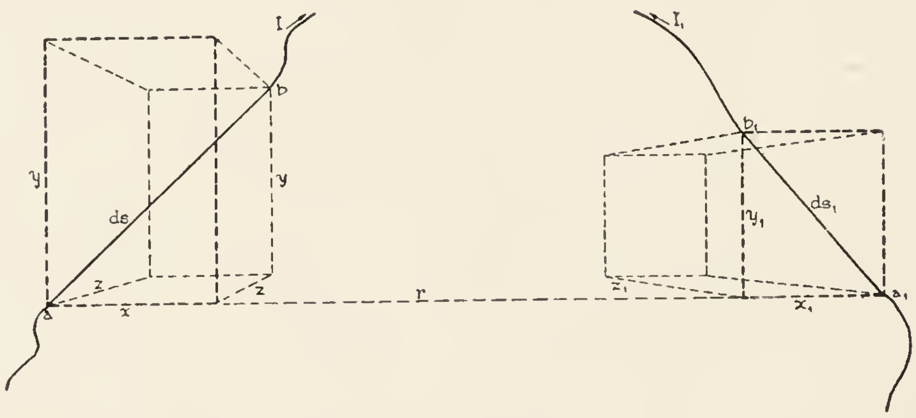


Fig. 8. Elements of Two Circuits, General Case with Reference to the X, Y, and Z Axes.

the *sense* of the current. Ampère began by seeking for an expression for the force of attraction or repulsion between two infinitesimal portions, ds, ds_1, of the conducting system under investigation, taking into account the three dimensions of space and the distance between the elements (Fig. 8). From experiments upon conductors of finite length, carrying currents I, I_1, he showed that there are four cases or conditions of equilibrium, and from these he was able to express the attractions and repulsions in mathematical form, involving coefficients.

Denoting by α and β the angles which ds, ds_1, respectively, make with the line joining them, and by γ the mutual inclination of the planes containing, respectively, the angles α and β, he was able to express the mutual action of the two elements by:

$$\frac{I\,I_1\,(\sin\alpha\,.\,\sin\beta\,.\,\cos\gamma + K\cos\alpha\,.\,\cos\beta)}{r^n}$$

where n and K were constants to be determined by experiment. He was unable to determine K in time for his *Communiqué à l'Académie* of December 11, 1820, and he did not then realize that K is negative. Ultimately he identified the coefficients, and he then expressed the required force as

$$\frac{I\,I_1\,(\sin\alpha\,.\,\sin\beta\,.\,\cos\gamma - \frac{1}{2}\cos\alpha\,.\,\cos\beta)}{r^2}$$

From this brief survey of his crowning achievement, it is seen that he concerned himself primarily with the geometry of the problem. For his purpose it sufficed that directed forces acted between particles across an abyss. With what substance, imaginary or real, that intervening space was filled was, to him, of minor importance. Nevertheless, his conception that molecules subjected to the action of continuously circulating electric currents might be regarded as equivalent to magnets was a significant step in that advance of knowledge relating to the constitution of matter which has led the modern school to the equations of free space, and to the present electron theory.

If from this point, the highest in his scientific career, the rugged ground over which Ampère trod is contemplated, the impression upon the mind is that during those dreary years he moved consciously towards a definite object. He never drifted. Inspired by a passion for research, and guided by his genius, he concentrated upon a single purpose. Once before he had been near to success and had just missed his mark; here at last was a reward in full measure. On the occasion of his previous disappointment he wrote (September 3, 1814) to his old friend Ballanche, from Paris:

> Mon ami, je n'aurais jamais dû venir à Paris. Pourquoi ne suis-je pas resté toute ma vie professeur de chimie à Bourg ou à Lyon! . . . Heureux ceux qui cultivent une science à l'époque où elle n'est point achevée, mais quand sa dernière révolution est mûre. La voilà faite entièrement par Gay-Lussac, qui termine l'ébauche créée par le génie de Davy, mais que j'eusse infailliblement faite. . . . Nous riions de si bon cœur à Lyon! Mais ici on ne rit pas.

Gay-Lussac had just published in the *Annales de Chimie* the results of his experiments upon Iodine. Ampère had examined

the nature of Iodine, but he was distracted by other work and other circumstances, and his discoveries concerning the element were not published in time to secure to him priority. It was at that stage that he decided to return to mathematics, which he had somewhat neglected in favour of chemistry. No schoolboy ever went back to his playground with more enthusiasm:

> Je vais me remettre aux mathématiques. J'éprouve une certaine difficulté d'abord, mais la première répugnance vaincue, le charme revient quand je puis écarter toute autre pensée et m'y consacrer uniquement, absolument uniquement; je voudrais ne plus quitter les calculs.

To such a research worker, the field was always ripe, and the glory lost in one direction by mischance was gained a thousand-fold in another by dint of the pluck and genius that never forsook him; for, as his correspondence proves, he laboured always with the same activity, the same fire, the same exaltation, the same carefulness—nothing so mobile as his ideas, nothing more persistent than his character.

It happens that by examining the biography of his English contemporary, Sir Humphry Davy, a significant side-light can be thrown upon his association with the early history of Iodine.

The fame of Ampère extended throughout Europe, and men of science were anxious to make his acquaintance. The circumstances under which Sir Humphry Davy visited him are recorded by John Ayrtoun Paris, F.R.S., who was a Fellow of the Royal College of Physicians and fully conversant with the facts. Incidentally the account reveals the magnanimity of the Emperor Napoleon towards men of scholarly attainments. Napoleon had sternly refused his passport to several of the most illustrious noblemen of England, but he gave it to Davy unconditionally.

Accordingly, Sir Humphry "accompanied by Mr. Faraday as secretary and chemical assistant", and Lady Davy, not to be outdone, "accompanied by her own waiting-maid", left London on October 13, 1813, and embarked for Morlaix in Brittany. At

the time of their arrival in Paris, Ampère was staying a few miles from the city, and consequently it was not until November 5, 1813, that the philosophers met. They became so engrossed in scientific discussion that Lady Davy—attended by her maid, and wearing "a very small hat, of a simple cockle-shell form" representing the *mode* in London but not in Paris—slipped out into the Tuilleries Garden, where she became the centre of such a crowd of Parisians that she was requested to retire. An officer of the Imperial Guard offered his arm; the throng became so dense, however, that it became necessary to send for a corporal's guard, and the party quitted the Tuilleries surrounded by fixed bayonets.

A few days later, the junior chemists, led by Thénard, gave Davy "a sumptuous dinner" in Paris; and it is significant of the relationship between the savants of the time, that "as it was by the chemists only that this dinner was given", neither Arago nor Ampère was included. What happened upon this occasion to "the secretary and chemical assistant, Mr. Faraday", is not recorded. It is of more importance to observe that on the morning of November 23, 1813, Ampère called upon Davy and placed in his hands a small portion of a substance which he had received from M. Clement. It had been in the possession of the French chemists for more than twelve months, but they had not determined its nature and composition. It was spoken of by them as "X, the unknown body", and it was produced in 1812 by B. Courtois, a manufacturer of saltpetre in Paris.

Courtois had shown it to Clement, who made some experiments without favourable result. Ampère received a specimen which he "carefully" folded in a piece of paper and deposited in his pocket. When he reached home it had vanished. Clement gave him another specimen and it was this that Ampère gave to Davy—for which generosity Thénard and Gay-Lussac were extremely angry with Ampère. Infinite credit is due both to the heart and mind of Ampère for this act of grace done in the grand manner of a great Frenchman, and it supports the statement of Davy's biographer that nothing ever exceeded the liberality and unaffected kindness and attention with which the savants of France received the English philosopher.

The literature appertaining to Ampère reveals him in many aspects. In the *Nouvelle Biographie Générale*, vol. ii., pp. 403-415, there is a concise account of his life, and a list of his Memoirs from 1802 to 1827. In the *Revue des deux Mondes* for February, 1837, there are two appreciative articles concerning him by Sainte-Beuve and by the younger Littré. Arago's account of him in the *Galerie des Contemporains illustres*, vol. x., is classic. A special number of the *Revue Générale de l'Électricité* for November, 1922, contains a series of articles by MM. Marcel Brillouin, P. Appell, A. Pérot, J. Pomey, H. Coquet, and M. P. Janet, on various aspects of his achievements and history. A book by Louis de Launay, published in 1925, under the title *Le Grand Ampère*, is a sympathetic review of Ampère's life and work, and touches gently on the mistake of August, 1806, when he married Mademoiselle Potot, from whom he was separated in 1809. Beyond these sources of information there is his Correspondence, and there is also a book which as a human document surpasses them all, entitled *La Vie et les travaux d'André Marie Ampère*, by C. A. Valson (1886). Add to these the works of De la Rive, and it will be realized that in literature the name and fame of Ampère are well guarded. His electrodynamic theory receives extensive treatment in the works of Maxwell, and it provides Tait with a theme in quaternions. (See Tait, *Quarterly Journal of Mathematics*, 1866; and *Quaternions*, paragraph 399. See also Forbes, *Philosophical Magazine*, February, 1861).

Observing his work as a whole, it is seen that at different periods of his career he gained world-wide distinction in different fields, the first of which was mathematics. His early efforts in physics, as De Launay has pointed out, were comparatively non-productive. He perhaps realized this, for as the result of his conversations with Davy he proceeded to make a name as a chemist. Nevertheless in 1805 he confessed:

> Je m'occupe plus que jamais de métaphysique . . . combien est admirable la science de la psychologie . . . la seule chose qui m'intéresse encore.

His versatility, and the temperament that caused him to

allow his mind rapidly to wander from one subject to another, however, could be brought under control. The surface ripples were unable to divert the steady undercurrent of concentrated thought that led to such results as his electro-dynamic theory. Mathematics kept him to the track.

It is usual to associate Ampère with the invention of the electric telegraph, but it is more appropriate to think of him in this respect as one of a large group who contributed at the initiation of the idea. Ampère directed attention to a notion put forward by Laplace that it might be possible to cause a magnetic needle to deflect at a very great distance by using long conductors with a battery in circuit. The chief proposal of Ampère was to provide a number of such circuits and magnetic needles, each identified with a letter of the alphabet, and by this means to spell out messages. A suggestion of greater practical importance to telegraphy, however, was Ampère's astatic galvanometer.

Although plain living and high thinking had been his rule, Ampère in 1824 was on the border of bankruptcy. His sister Joséphine who had tried to maintain his domestic budget in equilibrium and to protect him from household cares, was at last obliged to confess to a negative balance:

> Ma pauvre sœur . . . m'a caché pendant près de cinq ans des déficits . . . au total 4000 francs de dettes.

He had expended most of this sum upon instruments for his experiments. For about ten years he had been troubled also with rheumatism. To complete his discomfort, critics were busy. Physicists in France were suggesting that his theories were opposed to Newton's laws. In fact, Ampère, at the dawn of the era he created, had to confess that of all the members of the Académie the only one that received his theories favourably was Fourier.

Beyond the borders of France, criticism at one time was even more severe. Unable at first to follow the analytical deductions, Davy, Faraday, Seebeck, De la Rive, Prévost, Nobile, and a host of others raised objections. The truth is that until Babbage returned from a visit to Paris where he had an oral

explanation, Ampère was not understood in England. This explanation to Babbage destroyed all misconceptions, and led to the complete triumph of Ampère, but the triumph was somewhat late. He had lost all illusions about life. He knew at last the meaning of detachment. He had also learned that:

> Le vrai but de la politique ne doit pas être de rendre les hommes heureux, mais de les rendre meilleurs. . . .

He had even learned to cast aside the credulity that says:

> J'aime mieux le croire que d'y aller voir.

To posterity, therefore, he left the task of combining happiness with betterment, and analysis with faith.

Thanks to the courtesy of the Librarian of the "Bibliothèque de la Ville de Lyon" it has been possible to obtain copies from the original prints of portraits of Ampère. The reproduction at the beginning of this chapter is from a lithograph of a drawing from life by Boilly. Fig. 9 is from an engraving of a drawing from life by Ambroise Tardieu.

When searching at the Bibliothèque for these portraits for the present story, there were found by good fortune some manuscripts (Lyons Library, References 15653, 15670, 1102) of exceptional value, one page of which is reproduced in Fig. 10. It is part of the draft of his famous Mémoires of 1823 and 1824 on the mutual action of two conductors.

That Ampère disliked writing may be inferred:

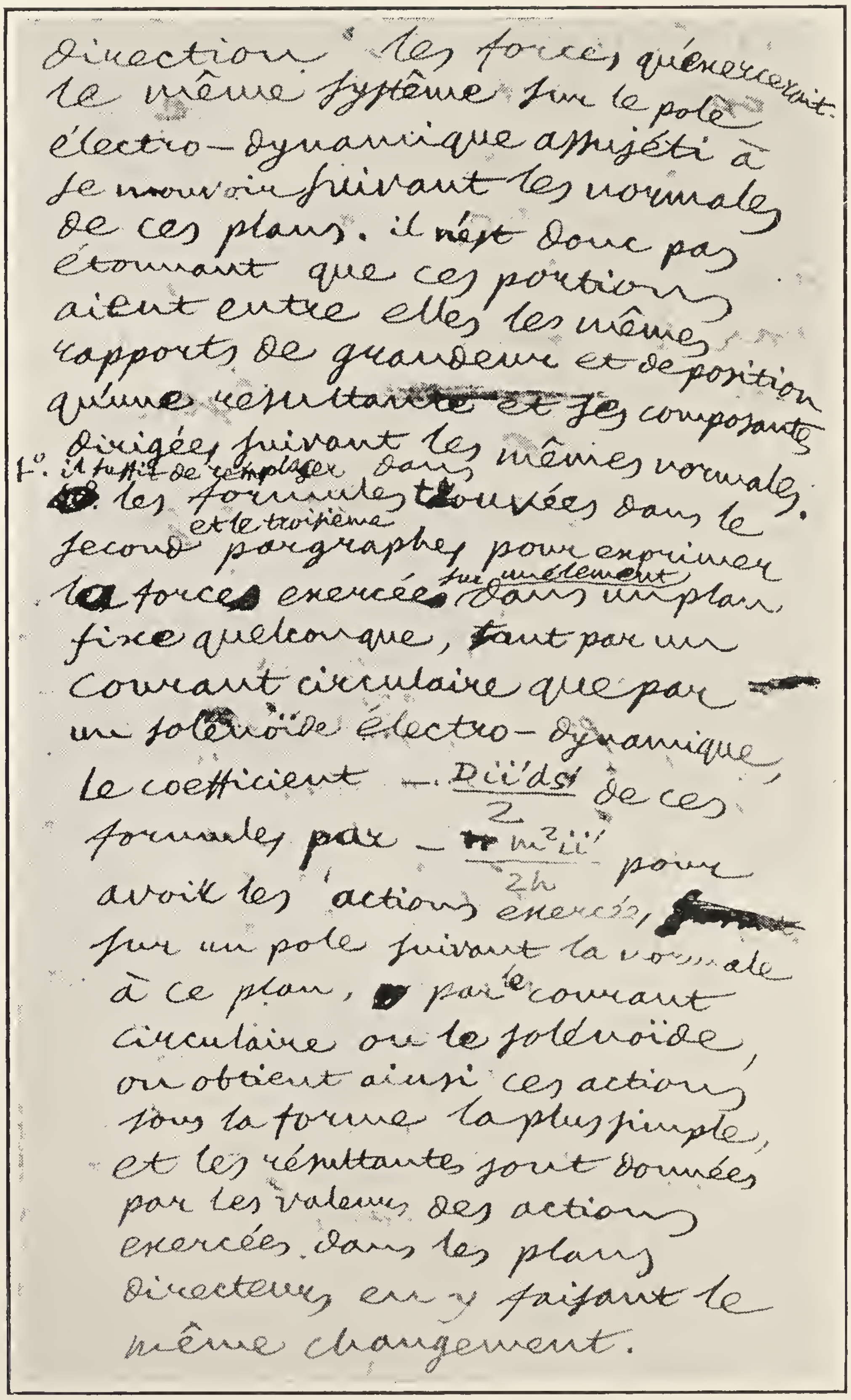
> Être assis, écrit-il, devant une table une plume à la main, c'est le plus pénible, le plus rude des métiers.

He could obtain no inspiration when seated; he preferred to stand up or to walk about when thinking. He suffered from short sight, and accordingly he wrote in somewhat large characters. His duties as Inspector-General of the University obliged him to move about France a good deal, and it was his custom to associate his ideas with the places where they originated in his mind: The Theory of Avignon, The Demonstration of Grenoble, The Proposition of Marseille, The Theorems of Montpellier.

His intention was to establish psychology as a science for all time. The attractiveness that it had for him was occasionally

FIG. 9. PORTRAIT OF AMPÈRE. From an Engraving of a Drawing from Life. By Ambroise Tardieu.

overpowering. This explains why a letter to him from Davy upon physics and chemistry received no reply:



direction des forces qu'exerceraient
le même système sur le pole
électro-dynamique assujéti à
se mouvoir suivant les normales
de ces plans. il n'est donc pas
étonnant que ces portions
aient entre elles les mêmes
rapports de grandeur et de position
qu'une résultante et ses composantes
dirigées suivant les mêmes normales.
1°. il suffit de remplacer dans
les formules trouvées dans le
second et le troisième paragraphes pour exprimer
la force exercée sur un élément ~~dans~~ un plan
fixe quelconque, tant par un
courant circulaire que par
un solénoïde électro-dynamique,
le coefficient $-\frac{Dii'ds}{2}$ de ces
formules par $-\frac{m^2 ii'}{2h}$ pour
avoir les actions exercées,
sur un pole suivant la normale
à ce plan, par le courant
circulaire ou le solénoïde,
on obtient ainsi ces actions
sous la forme la plus simple,
et les résultantes sont données
par les valeurs des actions
exercées dans les plans
directeurs en y faisant le
même changement.

FIG. 10. AMPÈRE'S MANUSCRIPT AT THE BIBLIOTHÈQUE DE LA VILLE DE LYON.

. . . n'ayant plus le courage à fixer ses idées sur ces ennuyeuses choses là.

This passion for psychology was balanced somewhat by the fascinations of chess. His working room was open to all, but it was difficult to get out of it without playing. His friends, when in a hurry, would adopt the low tactics of challenging his ideas upon electric currents and upon light-waves: and it is recorded that Ampère was then always "échec et mat".

André Marie Ampère died of pneumonia at Marseille on June 10, 1836, and was there buried. In 1869 his remains were transferred to Montmartre Cemetery. Thanks chiefly to the efforts of Mascart, the name of Ampère has been adopted universally as the designation of the unit of electric current, and thanks largely to Joubert his memoirs have been reproduced for posterity. Thus his works follow him. They constitute the proper memorial of a philosopher. Yet, if there is ever to be a temple of scientific research in Paris, devoted in the noblest sense to the welfare of mankind, France may appropriately write across its portal the name of Ampère, for his gifts to humanity can be repaid only in contributions that further the purpose for which he lived.

Portrait of Volta by Giovita Garavaglia, reproduced from Elogio Scientifico di Alessandro Volta, which was published at Como in 1834.

III

ALESSANDRO VOLTA

Alessandro Giuseppe Antonio Anastasio Volta was born on February 18, 1745. His descent can be traced to Zanino Volta of Loveno, and to Martino Volta who in the year 1500 was a merchant of Venice, trading in wool on the Rialto. The parents of Alessandro were Phillippe Volta and Madeleine de Conti Inzaghi. With a happy disposition, great powers of application, a sense of order, and his father's guidance in his studies, he soon attained an enviable place amongst his fellows at the public school of Como. The Royal Seminary at Como put the finishing touches upon his education, and so far as can be judged, his early leanings were towards prose and verse. By the time he was twenty-four, however, chemistry and electricity cast upon him spells that caused Italy to lose a poet, and natural science to gain a pioneer.

Volta was not destined to be merely a chemist and a physicist; he became a man of affairs who directed the thoughts of Europe to noble purposes. In 1777 he travelled to Switzerland, and met De Saussure, Voltaire, and other men of thought and distinction. Two years later he was called to occupy the chair of physics at Pavia. In 1780 he visited Bologna and Florence. In 1782 he proceeded to Germany, Holland, England and France, to confer with such intellectual giants as Lichtenberg, Van Marum, Priestley, Lavoisier and Laplace, and incidentally to enrich the cabinet at Pavia with instruments of research and demonstration.

Volta married on September 22, 1794, at the age of forty-nine, Donna Teresa Perigrini Ludovico. During the next five

years, he wrote some of his most valuable memoirs, and secured universal fame.

To realize the conditions under which, at this time, he was carrying out his researches, it must be remembered that all Europe was at war, and that his home at Como was at the very vortex of the tempest that raged between Austria, France and Italy. In 1799 the Austrians dominated the passes of the Alps, and they determined upon an attack on Genoa. The effort was successful, but it was made at the expense of their control of the approaches to Italy. In May, 1800, Bonaparte took advantage of the strategic situation thus created, and invaded Lombardy. With 35,000 men and 5,000 horses, he traversed the narrow pass of the great St. Bernard; he occupied Aosta, he lost and then won the battle of Marengo (June 14, 1800), he conquered Lombardy, and five years later (May 26, 1805) he was crowned at Milan. His entry into Italy in 1800 was greeted with rejoicings; for the educated classes sought liberty and expected it from the French, who were regarded as deliverers, and it was the policy of Bonaparte to come to terms in order to permit of concentration elsewhere. The extinction of the Italian republics, and the reinstatement of Victor Emanuel at Piedmont, followed the swing of the European pendulum in 1814.

Time, place, and circumstance conspired to establish friendly relationships between Volta and the First Consul. In 1801, at the invitation of Bonaparte, Volta visited Paris and gave a demonstration before a large meeting of the French Institute. Scarcely had it concluded when Bonaparte proposed that Volta should receive a gold medal. He also bestowed upon him 2000 crowns for his travelling expenses. Moreover, he decorated him with the Croix de la Légion d'Honneur and with the Couronne de Fer. He nominated him a member of the Italian Consulate, and he raised him to the dignity of Comte and Senator of the realm of Lombardy.

By perpetuating his name in the unit of electro-motive force, electricians long ago placed their laurels upon Volta. Testimony of his originality and of the scope of his activities is to be found in what remains of his apparatus, and in the literature that has been so carefully brought together in Italy

concerning his achievements. Unfortunately, a considerable amount of his original apparatus was destroyed in the disastrous fire that occurred at Como on July 8, 1899. Some of it was saved, however, and photographs exist of most of the instruments as they appeared before the calamity.

In the course of a recent visit to Italy it has been possible, through the courtesy of Professor Felice Scolari and of the Director, Mgr. Baserga, to examine these relics at the Museum in Via Giovio, Como, and to obtain the illustrations here reproduced. In addition, by the kind permission of Professor Francesco Massardi, it was possible to examine at the Reale Istituto Lombardo, in the Palazzo Brera, Milan, the original manuscripts of Volta and certain reproductions scrupulously prepared of essential portions of the apparatus as it existed prior to the fire.

Remarkable evidence of Italy's determination to do justice to the memory of Volta is to be found in *Le Opere di Alessandro Volta, edizione nazionale.* This work, in seven volumes, contains the copious private correspondence of Volta, relating to scientific and other subjects, and includes letters to such of his contemporaries as Priestley, Sir Joseph Banks, and Benjamin Franklin, together with the famous memoir of seventy-two pages written by Volta in the form of a letter to his friend Joannes Baptista Beccaria, Professor at the Turin University, in 1769, entitled *De vi attractiva ignis electrici*, containing the germ of the idea of the electrophorus. The famous *Raccolta Voltiana*, published at Como in 1899 as a tribute to Volta, includes an excellent series of illustrations of the apparatus, from which many of the figures for the present article have been prepared.

The age that followed him has confirmed the value of the discoveries associated with the name of Volta: the electrophorus, the Volta "pistol", the "lampada a gas", the eudiometer, electric signalling, the condensing electroscope, the apparatus for exploring the electric charges in the atmosphere by means of a flame, and the Volta pile in its various forms. At this distance of time, taking into account the lack of facilities that existed towards the close of the eighteenth century for

communicating and publishing ideas relating to physical science, it is necessary to exercise reserve concerning Volta's priority in relation to some of these items. In the main, however, the evidence in his favour with regard to most of them is sufficiently convincing to establish his right to be honoured as the inspiring genius who by incontrovertible experiment transformed electrical science from a polite amusement into a means of conferring limitless benefits of a practical character upon mankind.

These electrical investigations of Volta mark the transition from the old régime of electrostatics to the dynasty of galvanic action. At the age of eighteen he was in correspondence with the French philosopher, Jean Antoine Nollet (1700–70), who had distinguished himself in 1746 by sending a shock from a Leyden jar through 180 of the Royal Guards. Nollet had experimented upon pointed conductors, and upon the evaporation of fluids by electricity. He had also (1748) shown the relationship between lightning and the electric spark, and it was to Nollet that Etienne du Tour had addressed his account of the effect of a flame upon electrified bodies (1745). According to Arago the first electrometer (1749) is due to Darcy and Le Roy. Nollet suggested the more sensitive form in which two wires open like the legs of a compass; Cavallo added a small sphere at the end of each of the two wires, and Volta obtained increased sensitiveness by substituting a pair of dry straws.

In 1776 or 1777 Volta introduced his hydrogen-lamp (Fig. 1-1) and his electric igniter (Fig. 1-2). He was probably the first to ignite inflammable gases in closed vessels. His "pistol" (Fig. 2) dates from about the same time. It led to further experiments upon the ignition of inflammable gases by electric sparks, and it entered into his scheme for electrical communication. His proposal is indicated in Fig. 3, which was prepared from a sketch sent by him to his friend Barletti, Professor of Physics at the University of Padua, to show how a signal might be transmitted electrically from Como (left) to Milan (right). An iron transmission wire A B was to be supported on insulating posts *a*, *b*, *c*, *d*. The return circuit was to be through the canals

and streams shown in Fig. 3 above the transmitting wire. A Leyden jar discharged into the transmission line at Como was to detonate a "pistol" at Milan.

It is right to add that the principle of such a method of electrical communication was known long before 1776. In the *Philosophical Transactions of the Royal Society for* 1745, volume 43, William Watson gives an account of a method of firing bodies electrically, and he refers to the ignition of gas by electric sparks. Moreover, in those *Transactions for* 1748, volume 45,

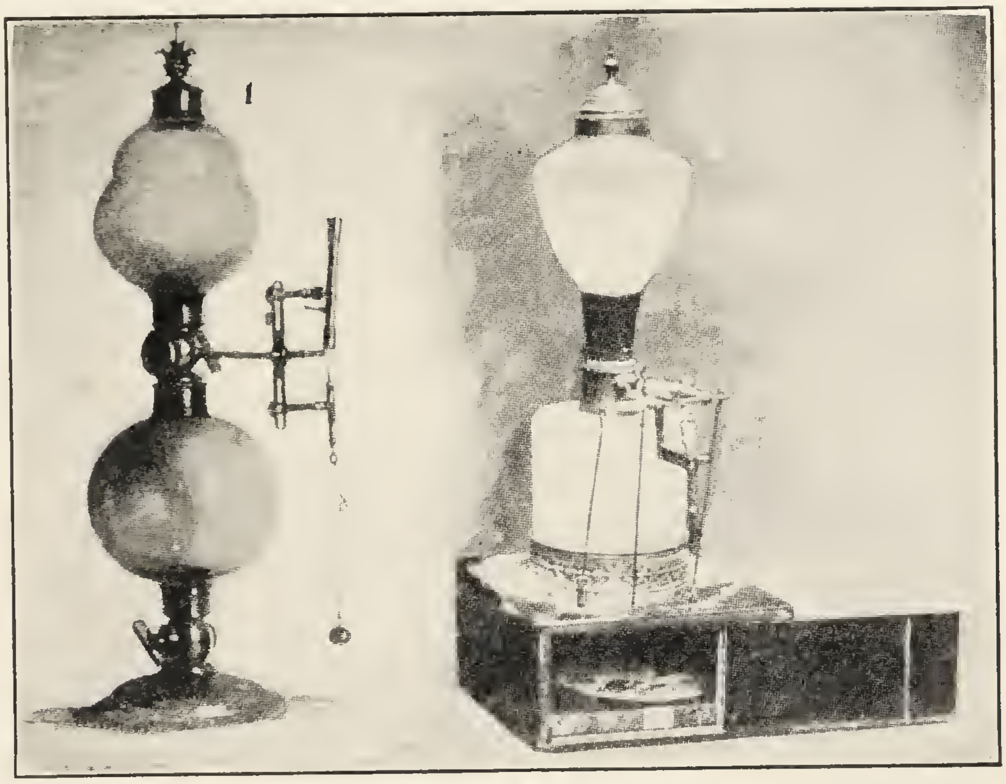


FIG. 1. VOLTA'S HYDROGEN LAMP (1), AND ELECTRIC IGNITER (2).

there is a description of experiments in which certain Fellows of the Royal Society contrived to make the "shock" from a Leyden jar traverse a circuit of two miles, and in one case of nearly six miles. They also succeeded in signalling in this manner across the Thames at Westminster Bridge. In the same volume there is further a reference to a paper concerning electrical experiments closely resembling these, by "an ingenious gentleman, Mr. Franklin".

The evolution of the electrophorus is somewhat difficult to trace. In England, John Canton (1718–72) established the principle of electrostatic induction. In Stockholm, Johann Karl Wilcke (1732–96), and in Berlin, Franciscus Alpinus

(1724–1802), found that a plate of air could be charged like a plate of glass. Volta applied the principle of electrostatic induction, and designed the electrophorus with such knowledge and skill that no improvement upon it has been made.

At about the time he had perfected the electrophorus, it may be assumed that he was perplexed by the question of the

Fig. 2. Volta's "Pistol".

best means of disclosing to the scientific world ideas that had matured in his mind, and that he desired to ascertain what was known elsewhere concerning his subject and kindred matters. In the spring of 1782 he journeyed to England. He examined the manufacturing centres, the canals, and the harbours. He saw Oxford University and Blenheim Castle, the factories of Birmingham and Manchester, and the salt mines of Nantwich, and he included in his tour Chester, Shrewsbury, Bridg-

north, Worcester, Gloucester, Bath, Bristol and Liverpool. At Portsmouth he visited the Fleet, under Admiral Howe, of twenty ships of the line, and went on board H.M.S. *Regina* of ninety-eight guns; and at Greenwich he went over the Royal Observatory.

During this stay in England he became acquainted with Dr. Joseph Priestley (1733–1804), Sir Joseph Banks (1743–1820) and other Fellows of the Royal Society, with the result that on March 14, 1782, a paper by "Mr. Volta, Professor of Experimental Philosophy in Como", was communicated to that Society by the Right Honourable George Earl Cowper, F.R.S.,

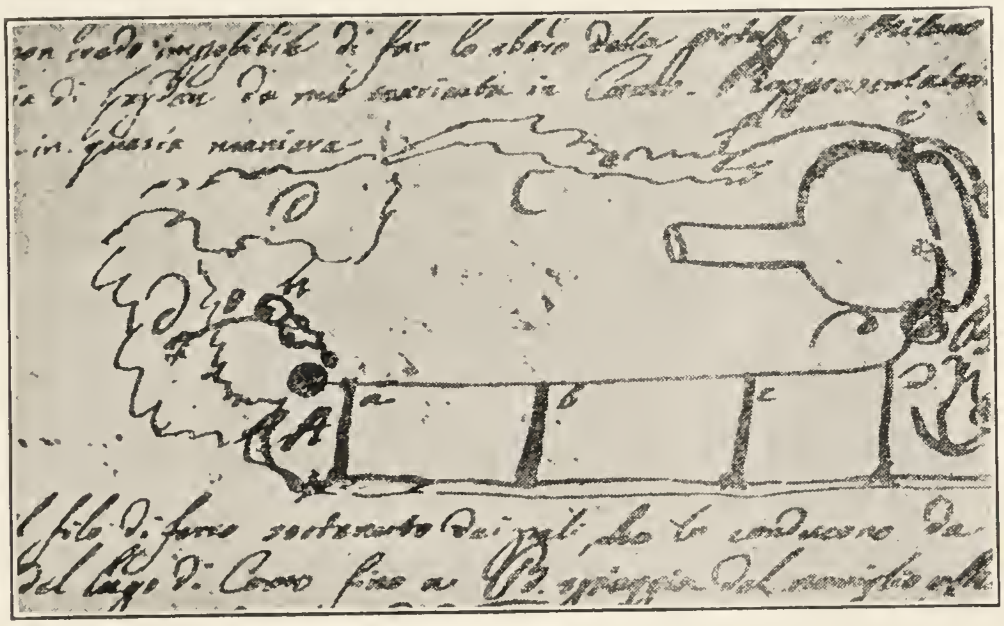


FIG. 3. VOLTA'S "PISTOL". Applied for Receiving a Signal.

and was published in the *Transactions* of that year, volume 72, p. 237 under the title, "The Method of Rendering Very Sensible the Weakest Natural or Artificial Electricity". Volta there describes his electrophorus as "a machine well known to electricians". He states that it might better deserve the name of "electrometer" or "micro-electrometer", but that he preferred to call it a "condenser of electricity".

It appears, therefore, that he recognized it as primarily an apparatus for facilitating the measurement of electric charges, rather than as a generator of static charges. In this manner he associated it with his condensing electroscope—which could be used for detecting extremely small charges—and thus was able

to show clear signs of electricity accompanying such processes as evaporation of water, the combustion of coal, and the effervescence of "iron filings in dilute vitriolic acid".

The illustrations in Figs. 4, 5, and 6 are evidence of the excellence of the designs perfected by Volta in the electrophorus and the condensing electroscope. He advocated the use of a very thin (*e.g.* 1/50 inch) resinous coating, and he insisted upon full and flat contact between it and the metal plates. Time must be allowed "till the metal plate may have acquired a sufficient quantity of electricity".

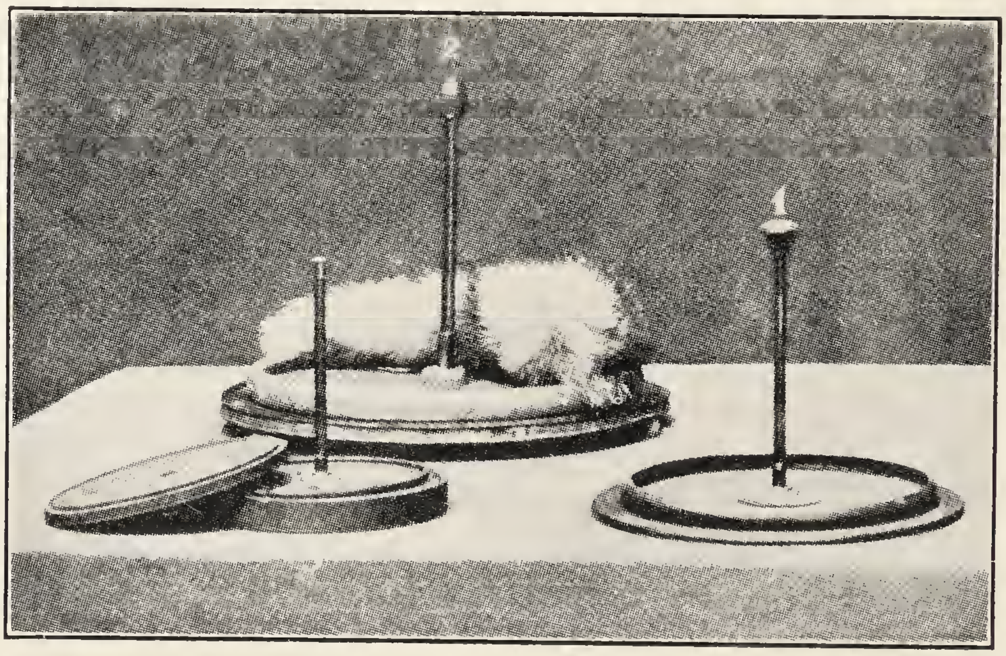


FIG. 4. THE ELECTROPHORUS. Said to have been invented by Volta in 1775. It gave him world-wide fame. Attention was directed to its importance by Dr. Priestley.

For meteorological observations, the "atmospherical conductor", which De Saussure added to Cavallo's electrometer in 1785, presumably an apparatus like Fig. 7, and to which Volta added a flame, was to have "the fewest joints possible". Further observations recorded in this famous communication by Volta are to the effect that: (1) By slightly melting the surface of the resin in the sun, it loses all its charge; (2) The flame of a candle or of a piece of paper will discharge it; (3) Fine hair is a sensitive detector; (4) Fusion, or a strong degree of heat, renders every body a conductor of electricity; (5) The augmentation of the phenomena is greater in proportion as the body which supplies the metal plate of the condenser with

electricity has "a greater capacity"; (6) The electricity "cannot be accumulated beyond a certain degree", *i.e.*, beyond the condition when it begins to be dissipated; (7) To avoid the

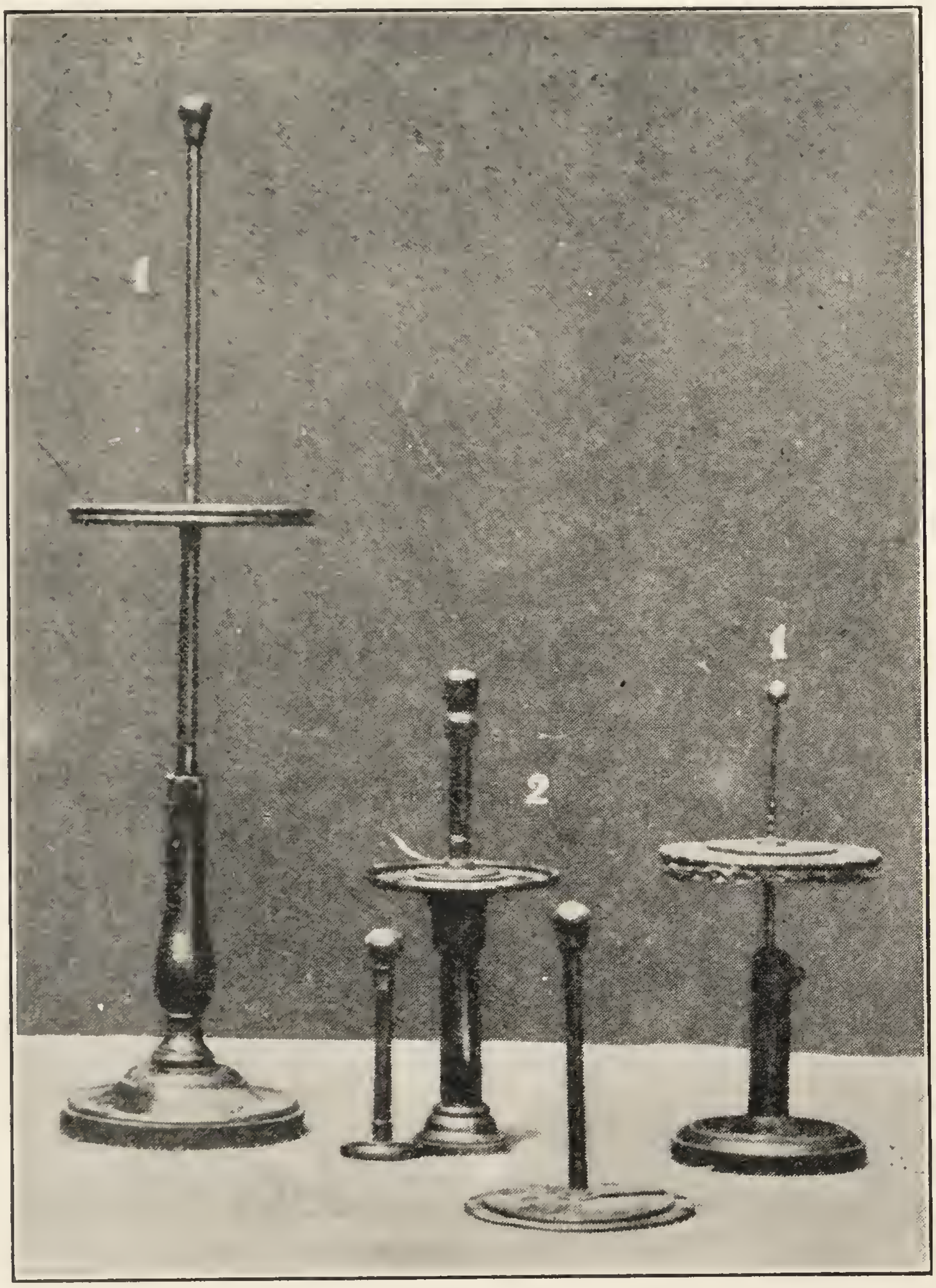


FIG. 5. VOLTA'S CONDENSING ELECTROSCOPE (1). The condenser (2) is said to have been charged by the evaporation of liquids. These are from the Istituto Lombardo.

inconvenience in certain experiments of the resin retaining its charge, it may be useful to try partial conductors like wood or marble which do not "contract" electricity; (8) For this purpose, old marble was found to be better than new; (9) Surfaces should coincide so well as to exhibit cohesion; (10) Copal, amber or lac varnish is recommended on marble, or on a truly

flat metal plate; (11) Cloth, satin, silk, paper, leather, ivory or bone, if well dried, may be used for condensers; (12) A Leyden phial which appears to be deprived of its charge when the coatings are momentarily connected, gives further sparks if allowed time to recover; (13) "Capacity is inversely as the intensity, by which word I mean the endeavour by which the electricity of an electrified body tends to escape from all parts of it"; and (14) The capacity of two conjugate conductors

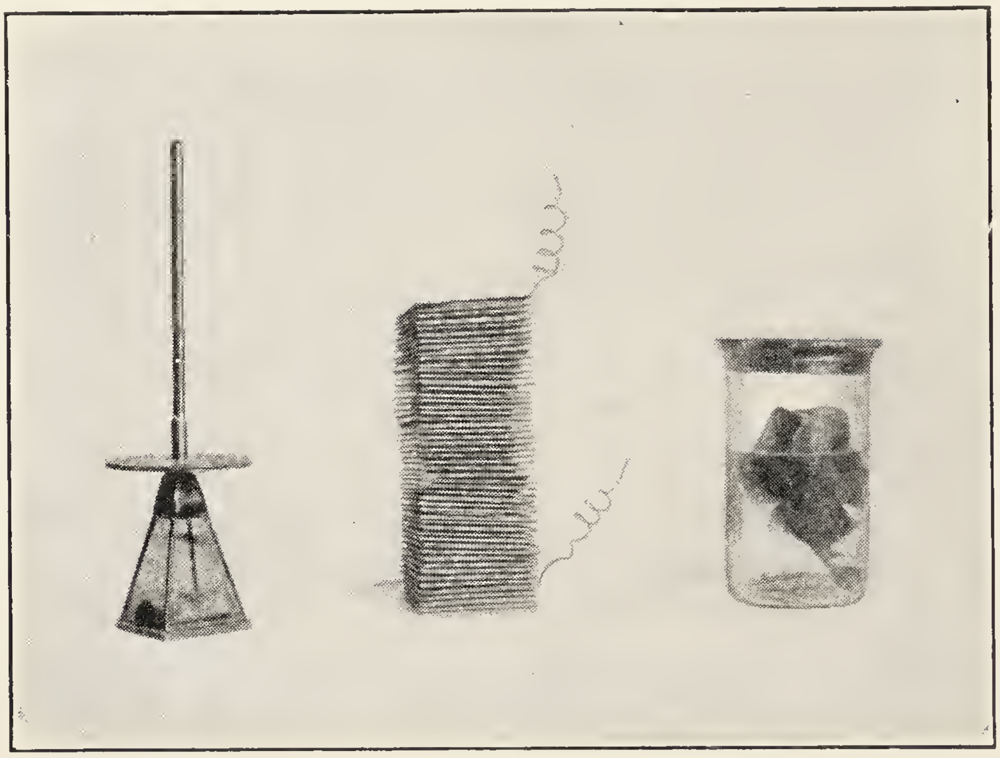


FIG. 6. VOLTA APPARATUS PRESERVED AT THE UNIVERSITY OF PAVIA, including a Condensing Electroscope (1), a Pila of square form, and a Terpedine preserved in alcohol.

increases as they are brought nearer together, the quantity meanwhile remaining constant except for leakage and the intensity diminishing.

Arago describes Volta's condensing electroscope as a veritable microscope, and in the hands of the pioneers, it has been a valuable aid to quantitative research. There is satisfaction in recording that Volta was elected a foreign Fellow of the Royal Society in 1791, chiefly on account of the discoveries disclosed in his communication of 1782, an acknowledgement which encouraged him to march forward to experiments that revealed him in the domain of physical science a king in his own right.

The state of electrical knowledge before 1782 may be

gathered from the communication by the Honourable Henry Cavendish, contained in the *Transactions of the Royal Society*, volume 66, 1776, p. 196, read on January 18, 1775, with the

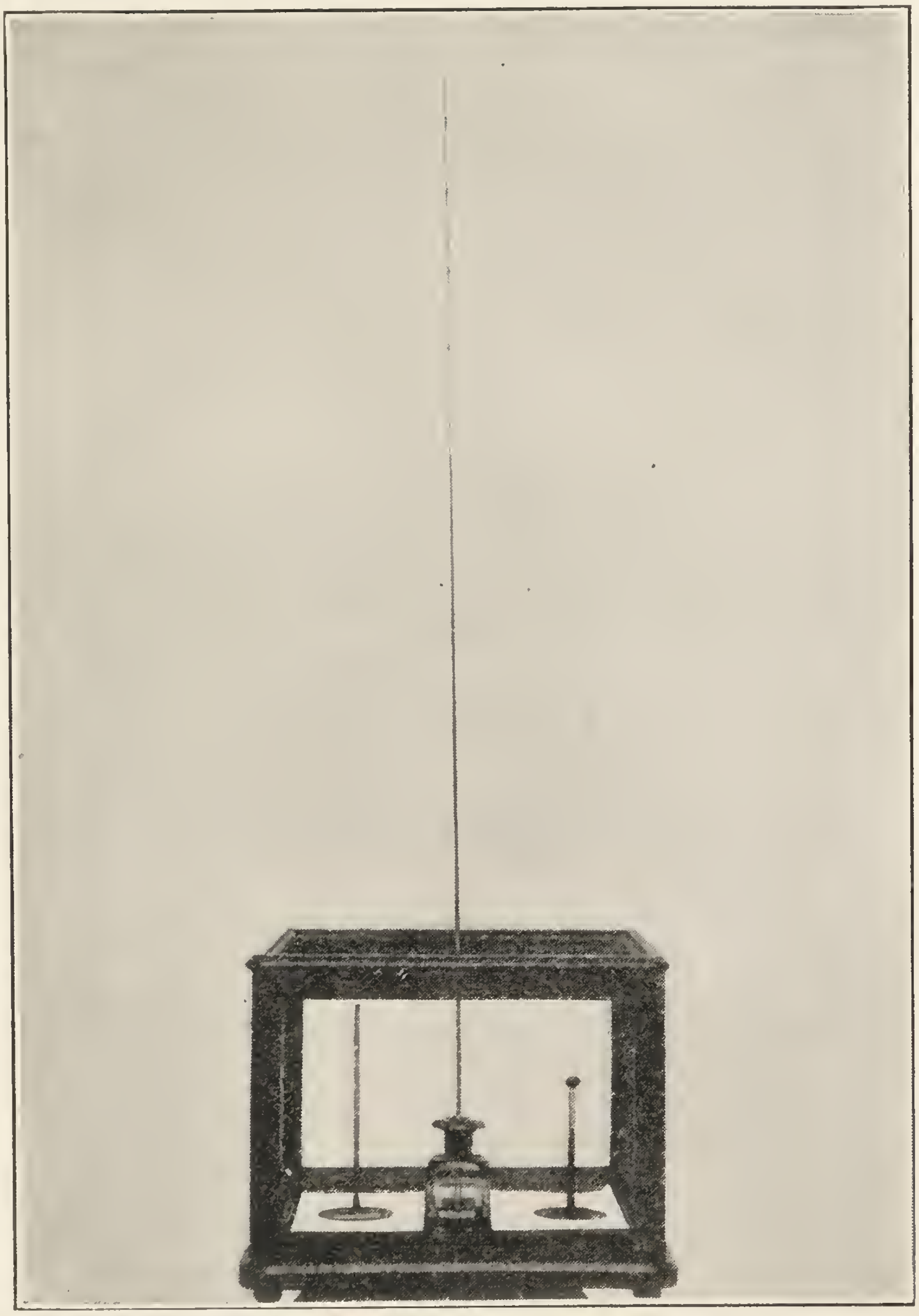


Fig. 7. Volta Apparatus for Investigating Electric Charges in the Atmosphere.

title "An Account of Some Attempts to Imitate the Effects of the Torpedo by Electricity". Cavendish used 49 Leyden jars of extremely thin glass, disposed in seven rows. He compared their capacity quantitatively with that of a plate condenser of crown-glass dielectric of known dimensions. He found the

capacity of a condenser to be directly as the area of the coating and inversely as the thickness of the glass, whereby the proportion of the quantity of electricity in the jars to that in the plate-condenser was computed by charging to a definite difference of potentials as determined by his somewhat crude electrometer. He calibrated the electrometer by dividing the charge of one jar between two similar jars. He then charged a row of Leyden jars, and observed how many times it was necessary to charge the plate condenser from the row of jars in order to reduce the potential of the row to one-half the initial value.

FIG. 7*a*. COMO, where Volta was born and worked and died. The house that was his home is at the corner of Via Alessandro Volta and Via Dell Annunciata. It bears upon a plaque the words: "Fu Questa L'Avita case de Alessandro Volta." (Formerly the dwelling-house of Alessandro Volta.)

Finally he deduced the ratio of the quantity of electricity in the row to the quantity of electricity in the plate, and by applying the logarithmic law he deduced "the quantity of electric fluid in the row". The fact that the theory of the condenser was known before Volta, however, in no way detracts from the value of his work on the condensing electroscope.

To close the account of this middle period of his career, it is appropriate to recall the words of his contemporary Thomas Thomson, the historian of the Royal Society who wrote:

We are more indebted to Mr. Volta than to any other philosopher of the present age for the introduction of new and important

electrical apparatus. In this Paper (*Phil. Trans. R.S.*, 1782) he gives us an account of his condenser; a very useful instrument for detecting the presence of small quantities of electricity. By means of it Lavoisier, Laplace, and himself succeeded in ascertaining the existence of negative electricity in the vapour of water, the smoke of burning coals, the air produced by the solution of weak sulphuric acid . . .

The years following 1782 were not spent wholly in Italy. With his friend Antonio Scarpa, Professor of Anatomy at Pavia, Volta went to Vienna, where the Emperor Joseph II received him in a most friendly manner. In 1784 he visited Berlin, and in 1787 he was at Geneva discussing French poetry with De Saussure. Then happened the stimulating event that, like a microscopical crystal falling into a super-saturated solution, converted the limpid facts and opinions of his mind into a brilliant notion. On August 30, 1789, Luigi Galvani of Bologna observed by chance the convulsions of a dead frog that was in contact with metal.

Galvani's memoir entitled *De viribus electricitatis in motu musculari commentarius* appeared in 1791, at a time when Volta was engaged upon his investigations upon inflammable marsh gas. On April 3, 1792, he wrote to Galvani concerning the frog movements, and a few weeks later he spoke in praise of the discovery. During the summer of 1792, however, he became dissatisfied with Galvani's explanation of the phenomena, and he concentrated upon the problem of contact-electricity.

It may be observed that the Royal Society awarded the Copley Medal to Volta in 1794, and that on the occasion of the award Sir Joseph Banks, the President said:

The experiments of Professor Galvani, until commented upon by Professor Volta, had too much astonished, and perhaps, in some degree perplexed many of the learned in various parts of Europe. To Professor Volta was reserved the merit of bringing his countryman's experiments to the test of sound reasoning and accurate investigation; he has explained them to Dr. Galvani himself and to the whole of Europe, with infinite acuteness of judgment and solidity of argument; and through the medium of the "Philosophical Transactions" he has taught us, that the various phenomena which presented themselves under the modifications

of Dr. Galvani's experiments hitherto tried, are wholly owing to the excessive irritability of the nerves when subjected to the actions of portions of the electric fluid, too minute to be discovered, even by the delicate electrometer of our ingenious brother, Mr. Bennet of Worksworth; and he has detected in the metals, which Dr. Galvani considered as mere agents in conducting his animal electricity, that very existing principle which the Doctor and his followers had overlooked.

Volta's work here deserves to be considered separately from that of the subsequent period, for he had not yet discovered the Volta pile. It is necessary first to introduce Tiberius Cavallo, an Italian physicist resident in England, who in 1775 published in London a treatise on atmospheric electricity. His observations were made at Islington. His book which went into several editions, described a multiplier, a condenser, methods of producing electric alarms, and other matters. He was responsible for improvements in the electrometer and was elected to the Royal Society. To Cavallo, Volta wrote, in French, giving an account of "Some Discoveries Made by Mr. Galvani of Bologna, with Experiments and Observations on them". The first part of Volta's letter is dated September 13, 1792. The second part, which is a direct continuation of the first, is dated October 25, 1792. This letter was communicated to the Royal Society, and it was published in the *Transactions* of 1793, volume 83, p. 10.

In a generous and prophetic phrase, Volta sums up in this letter his opinion of Galvani's memoir:

> Il contient une des plus belles et des plus suprenantes découvertes, et le germe de plusiers autres.

He explains Galvani's views, and he expresses the opinion that the electricity does not act immediately upon the muscles, but upon the nerves that excite the muscles. Again it was the instrumental value of the discovery that appealed to him—for here was an *électromètre animal.* He tried the effect of a two-metal probe upon detached portions of animals, and upon fragments, without any special preparation of the nerves. Then he proved that there was no need to suppose there was a discharge of electricity between nerve and muscle. With two probes of the same metal—alike in hardness, rigidity, polish,

and general surface, applied in the same manner—there was no convulsion.

His field of inquiry possessed no boundaries. He acquired intuitively the secret of successful experiment—the orderly extension, with a definite object, of all investigations to cases beyond the limits that convention may consider to be reasonable. Thus in these tests, though he failed with earth-worms, snails, and oysters to cause convulsions, he risked the opprobrium of anti-vivisectionists of future centuries, by extending his trials to other forms of life. Then he succeeded, and he was thus able to associate the action with the nerves of flexor muscles. His usual method in these tests consisted in decapitation, the insertion of a tinfoil strip into the neck, and the placing of a contact-plate of silver upon the body. He wickedly confesses to Cavallo:

> Il est fort amusant d'exciter de cette manière le chant d'une cigale.

He found that, whereas a specimen cut half an hour or even an hour after death, from a leg of a lamb and, therefore, almost cold and incapable of responding to any mechanical or chemical stimulus, was powerfully affected by an electric stumulus, a specimen from the heart removed from the same animal soon after death, still warm and, therefore, "très irritable", treated in the same way by the "arc-conducteur", exhibited no effect. He concluded that electricity does not stimulate the involuntary heart muscles but only those subject to the will.

In this communication (1793), Volta is careful to state that Galvani was the first to show that the electric stimulus of the nerves results in excitation of the muscles, although the electric current itself may not reach the muscles. He also describes an experiment in which he placed the bowl of a silver spoon on one side of his tongue and tinfoil on the other. He expected a movement, but he experienced instead a taste. Upon reflection he was reminded that the nerves of the tongue relate to taste and not to movement. The motor nerves are further back. Accordingly he tried the root of a lamb's tongue—the effect was to raise the point of it, and to cause it to turn one way or the other.

The basic conclusion at which he arrived as the result of this series of researches (1793) cannot be expressed better than in his own words:

> C'est véritablement une nouvelle loi bien singulière, que j'ai découverte; une loi qui n'appartient pas proprement à l'électricité animale, mais à l'électricité commune, puisque ce transflux de fluide électrique, transflux qui n'est pas au surplus momentané, comme serait une décharge, mais continu et suivi tout le temps que la communication entre les deux armures subsiste, a lieu, soit que celles-ci se trouvent appliquées aux substances animales vivantes ou mortes, ou à d'autres conducteurs non métalliques, mais suffisamment bons comme à l'eau, ou à des corps mouillés.

The earlier history of the effects of electricity upon animals probably extends to remote observations. Of recorded experiments, the most important is that contained in Sulzer's *Theorie der angenehmen und unangenehmen Empfindungen.* The copy in the British Museum Library is translated from the French and is dated Berlin, 1762. This account (p. 62) is literally as follows:

> Wenn man zwey Stücken Metall, ein bleyernes und ein silbernes, so mit einander vereiniget, dasz ihre Ränder eine Fläche ausmachen, und man bringt sie an die Zunge, so wird man einen gewissen Geschmack daran merken, der dem Geschmack des Eisenvitriols ziemlich nahe kömmt, da doch jedes Stück besonders nicht die geringste Spur von diesem Geschmacke hat. Nun ist es nicht wahrscheinlich, dasz bey dieser Vereinigung der beiden Metalle, von dem einen oder dem andern eine Auflösung vorgehe, und die aufgelöseten Theilchen in die Zunge eindringen. Man musz also schlieszen, dasz die vereinigung dieser Metalle in einen von beyden oder in allen beyden eine zitternde Bewegung in ihren Theilchen verursache, und dasz diese zitternde Bewegung, welche nothwendig die Nerven der Zunge rege machen musz, oberwähnten Geschmack hervor bringe.

After the publication of Volta's work of 1792–93, honours were bestowed upon him from all parts of Europe, but he was not entirely free from discordant incidents. The *Raccolta Voltiana* reminds us that in October, 1796, at Pavia, he was "publicamente insultato" because he was believed to be one of the principal supporters of a proposal to transfer the Univer-

sity of that city to Milan. From this accusation he defended himself in a written statement denying that he had favoured the transfer. Earlier in that year the municipality of Como had sent him in company with the Conte G. B. Giovio to compliment Bonaparte upon his entry into Milan, and he was admitted to the ducal palace and presented to the great Generale Supremeo. At the end of the year he applied, unsuccessfully, to be allowed to retire from the University of Pavia.

In February, 1797, Volta, Zola, Nani, and Presciani, dons of the University of Pavia, had the fortitude to protest against the Calendar proposed by the Minister of the Cisalpine Republic, which was the name given by the French to the conquered territory in Lombardy. This so much annoyed the Rector Rasori that he accused them, before the General Administration of Lombardy, of being Austrophiles—but without effect. In June, 1799, Bonaparte himself settled the question of Volta's position in the University of Pavia.

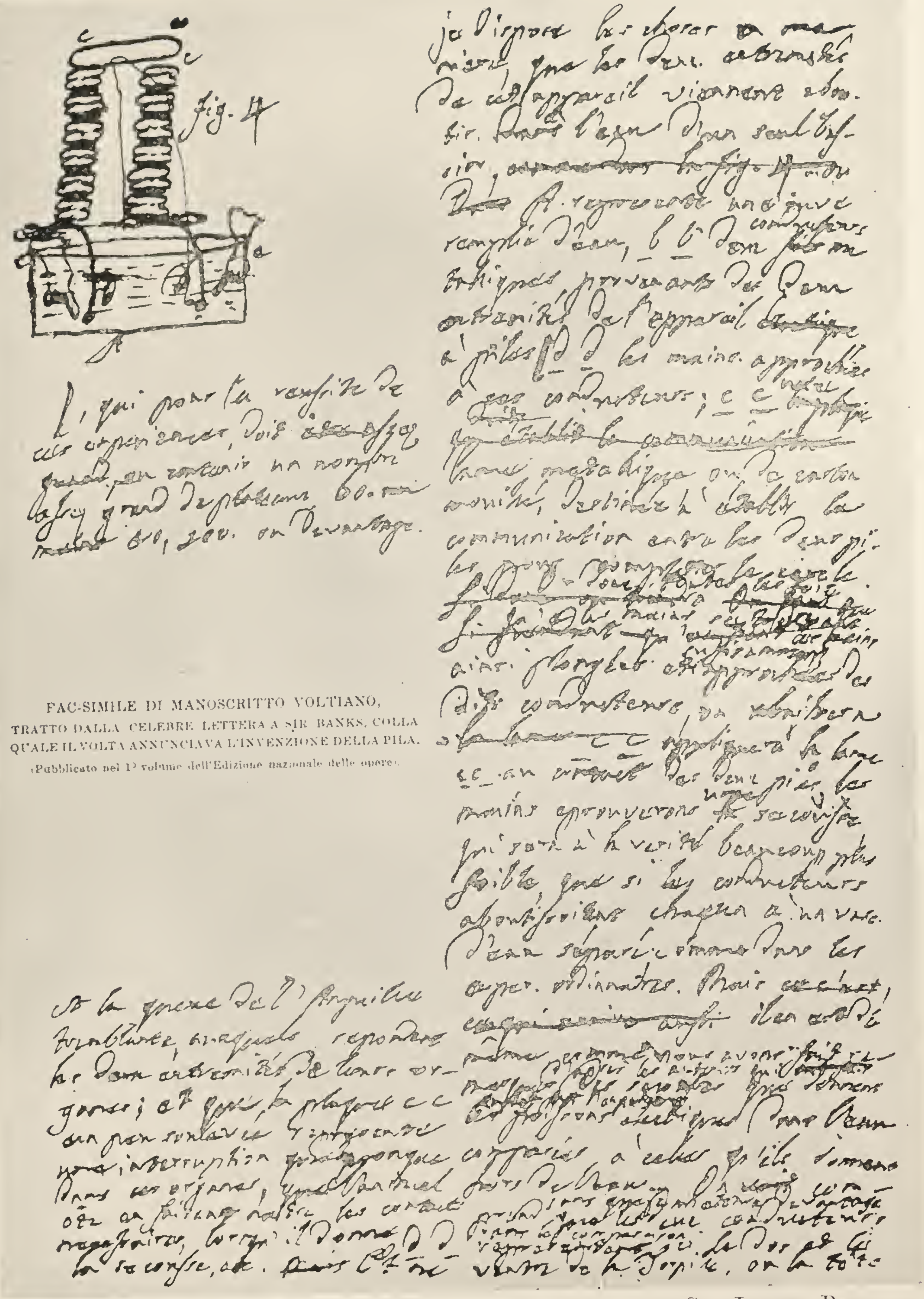
It is usual to associate Volta's conception of a continuous current with his discovery, probably towards the end of the year 1799, of the electric pile. The quotation from his letter to Cavallo, however, leaves no doubt that he had a vision of such a current in 1792, and that the production of a continuous flow became his objective. In making known to the scientific world the successful issue of these later investigations, he resorted to England as a medium of communication.

Volta's paper "On the Electricity Excited by the Mere Contact of Conducting Substances of Different Kinds" is published in the *Philosophical Transactions of the Royal Society*, vol. 90, 1800, pp. 403-31. It is written in French, and it takes the form of a letter addressed to the Right Honourable Sir Joseph Banks, Bart., K.B., President of the Royal Society. It was read on June 26, 1800. In the History of the Royal Society it is explained that owing to the state of hostilities that then existed between England and France, one portion of the paper arrived in London several months before an opportunity occurred for sending the remainder. This delayed publication of the discovery of the pile, and in consequence apparatus was constructed and various curious experiments by different

persons in London were made in advance of the original paper being laid before the public.

Briefly, Volta explained that he had been experimenting on the electricity excited by the simple mutual contact of metals of different kinds, and other different conductors, including liquids. The result was the construction of an apparatus—the pile—which in its effects resembled those from Leyden jars, but capable of giving a continuous discharge. Then he described the construction: a number of good conductors, 30, 40, or 60 pieces, preferably of copper, or better of silver, applied each to a piece of tin, or better of zinc, and an equal number of layers of water, or of some liquid which is a better conductor than water—such as salt water—or pieces of card or leather soaked with liquid and interposed between the metal discs, always in the same order. He pointed out that the apparatus resembles the "organe électrique naturel" of the torpedo rather than the Leyden jar, and consequently he called it the "organe électrique artificiel". Some of these are illustrated in Figs. 6, 8, and 9.

He took the shock or "commotion" through damp fingers, and he observed the need for good contact, as the force was very small. The effects were greater when the air was warm, as "heat renders water more conducting"; salt added to the water gave the best conducting effect, and the effect increased with the number of "pairs". With a view to reducing the size of the generating apparatus he designed the "couronne de tasses" (Fig. 10)—30, 40, or 60 goblets half-full of pure water, or better of salt water, containing metal arcs, copper-silvered and zinc. With these he tried various arrangements of connection, rows and columns, and he investigated the effect of reversals of polarity. Such an apparatus he called an "appareil électromoteur". He describes his sensation of commotion on contact —first "un coup et une piqûre", then "une douleur aiguë". He also analyses the sensation at the moment of interruption, due to "a kind of reflex action of the electric fluid". In this account he introduces the word "resistance"; he denies the existence of the "électricité animale" of Galvani, and he affirms that of "électricité extrinsèque", brought about by the mutual contact of metals of different kinds.



FAC-SIMILE DI MANOSCRITTO VOLTIANO,
TRATTO DALLA CELEBRE LETTERA A SIR BANKS, COLLA
QUALE IL VOLTA ANNUNCIAVA L'INVENZIONE DELLA PILA.
(Pubblicato nel 1º volume dell'Edizione nazionale delle opere).

FIG. 8. PART OF THE MANUSCRIPT ADDRESSED BY VOLTA TO SIR JOSEPH BANKS, MARCH 20, 1800, DESCRIBING THE PILA AND SHOWING VOLTA'S FIG. 4.

Like Cavendish before him and Maxwell after him, he was attracted to the problem of sensations caused by electrical action. He mentions that a plate of silver in contact with a plate of zinc produces an acid taste if placed upon the tongue, provided that the end of the tongue is towards the zinc, and a bitter taste (alkaline) if the metal plates are reversed—more so if an assemblage of plates is used, appropriately arranged. He found that when the current was applied to the eyes, the sensation of light was greater when a single couple was used than when 10, 20, or 30 were employed—with one electrode touching

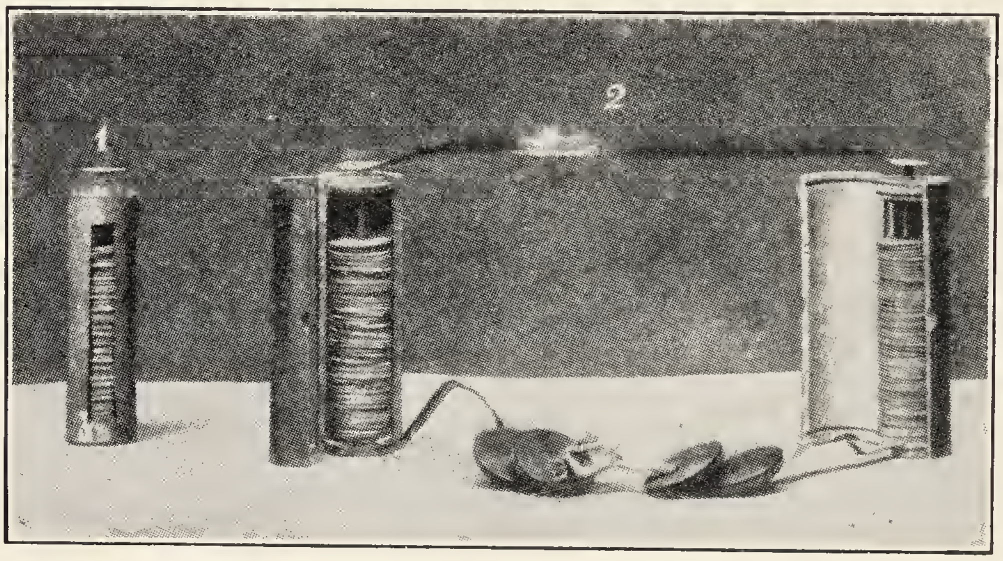


Fig. 9. Prepared Frog and Apparatus precisely as shown by Volta to Napoleon at The Institute of France in November, 1801.

the eyeball and the other held in the hand. The most curious of these experiments, he says, is to place one metallic plate between the lips and in contact with the end of the tongue, and to complete the circuit in any convenient manner. Then, if the apparatus is sufficiently large and in good order, a sensation of light will be experienced in the eyes, the lips will be convulsed, and the end of the tongue will feel a painful pricking sensation followed by the sensation of taste.

The adaptation of his own body as a testing equipment, however, did not end there. He described experiments in which the sensation of hearing (noise) is excited by sending a current through the ears from 30 or 40 couples. He describes the noise as "comme si quelque pâte ou matière tenace bouillonnait". He admits that the investigation was too painful and too

dangerous to be repeated. Lastly, he experimented with his nose, with the result that more pain was produced, but no sense of odour.

Arago, his contemporary, expressed the opinion that Volta's pile was the most marvellous instrument that human intelligence had ever created, and he might have added—the most mysterious. The discussion, which began in his day, regarding the seat of the electromotive-force, extends into the present century, and it is as far as ever from settlement. With a graceful tribute to Galvani, Volta referred to the effects as Galvanic

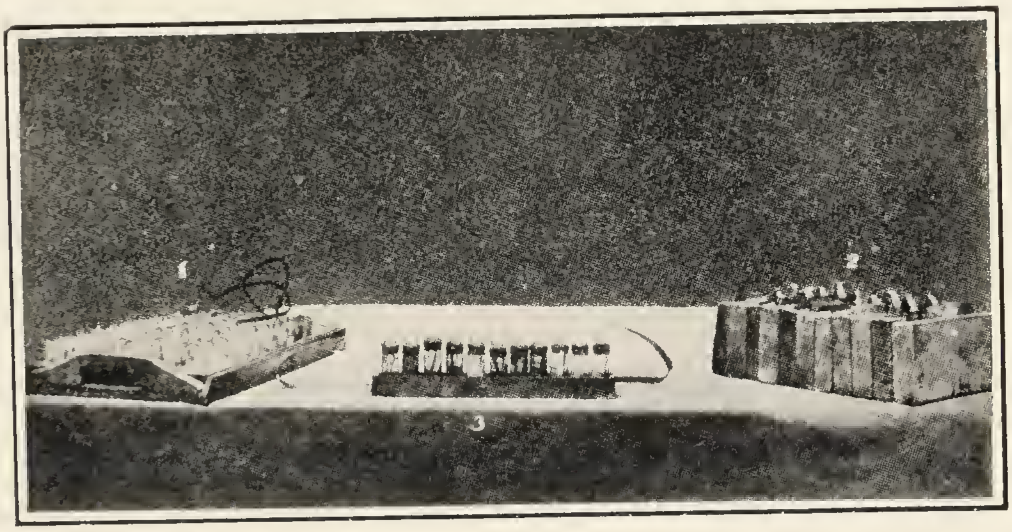


Fig. 10. Volta's Pila a Corona di Tazze (1). Volta's Pila a Truogoli (trough) (2). There is also shown the Pila Secondaria of Ritter. All from the Istituto Lombardo.

phenomena. In his reply to criticism he remarked that he had never attributed to the metals exclusively the power of causing the electric effect by their mutual contact. He had proved by many experiments that this power belongs to all conductors, that it is in general more marked between metals, but that it also manifests itself in the case of metals and moist non-metallic conductors. In his view, it was necessary to form the circuit of at least three conductors—say two of the first (metallic), and the third of the second (non-metallic) class; or two of the second and one of the first, or all three of the second but all different, as in the case of the torpedo.

He confessed that in his earlier experiments upon contact, he had used a prepared frog (Fig. 11) for the purpose, inasmuch as he had not perfected the means of measurement. He realized

that chemical as well as electrical action occurred, because in some cases gas was evolved, the metals became oxidized, and acid was produced. Similar effects had been obtained with static electricity. By arguments such as these, free from rancour, based upon direct evidence and couched in moderate language, he held his own and convinced Europe. The mystery of cause and location remained, but as to effects he left nothing in doubt.

It was natural that in the early experiments on contact electromotive-force Volta's condensing electroscope should be

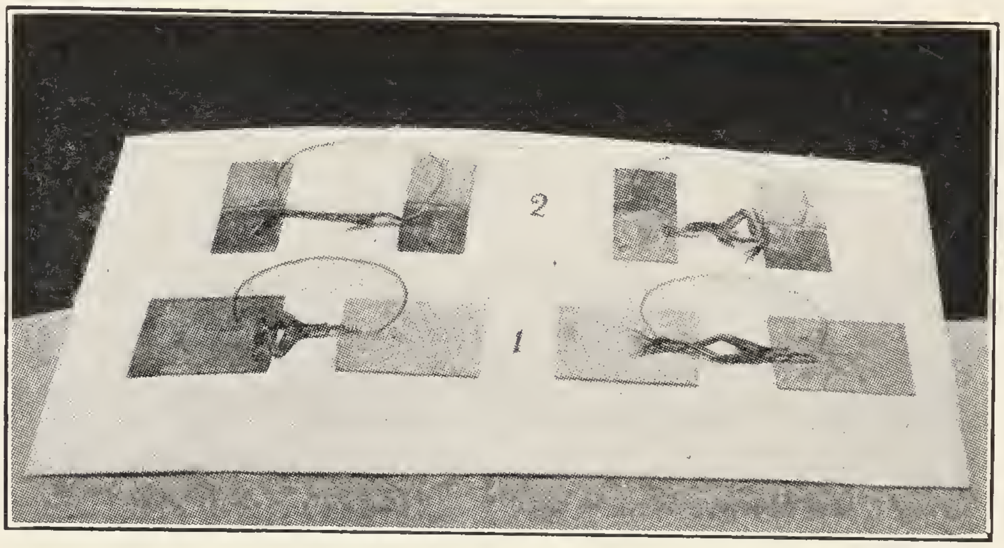


Fig. 11. Volta's Frog Preparations. These investigations were in the years 1792–1800.

requisitioned for demonstration purposes. John Cuthbertson, writing in 1805, describes an apparatus of this kind in which the copper plate is perforated with small holes, and is slightly dished. Zinc filings are then sifted through the holes and are allowed to fall upon "the top of the condensing electrometer". While sifting, the gold-leaves diverge, without help from the condensing plates, and are found to be positively electrified.

At the end of the eighteenth century and at the beginning of the nineteenth, the Volta pile in innumerable forms became the vogue, and experiments on nerve and muscle a fashionable amusement. Thus, for example, John Cuthbertson, writing in 1807, describes a demonstration in which the apparatus was arranged to produce contractions in the head of an ox, after separation from the body.

The eyes will open; the ears shake; the nostrils swell, and the tongue, which before hung out of the mouth, will be drawn in with violence.

Similarly for a dog, he explains that:

Frightful convulsions may be produced. The mouth will open, the teeth will gnash, the eyes roll in their orbits, appearing as if the animal was restored to life, and in a state of agony.

Sensationalism led to decadence, and yet Cuthbertson proclaims with pride:

These experiments I had the honour of performing in the presence of their Royal Highnesses the Prince of Wales, the Duke of York, the Duke of Clarence, and the Duke of Cumberland.

This evidence supports Whewell, who in his *History of the Inductive Sciences* states that during the twenty years that followed Volta's discovery of the pile, the impulse given by that invention to the study of electricity as a mechanical science nearly died away. In the laboratories of Europe, nevertheless, the investigation by direct experiment of contact theory had almost continuously engaged attention. Van Marum and Pfaff, of Kiel, led the way, and were joined by many others, among whom were Davy, Faraday, Kohlrausch, Pellat, Kelvin, Ayrton, Perry, and Erskine-Murray.

In this tribute to the great Italian philosopher, it must be recorded that although the mystery of contact-electricity, with which he endowed us, remains unsolved, it is still a potent incentive to research. Maxwell, Heaviside, and Kelvin were not all in complete accord concerning the action. The questions they were concerned with were: is there a metallic-junction force? When the two metals are in contact, is the air outside the zinc at a different potential from that of the air outside the copper? Is the seat of the electromotive-force at the air-surface, and if so what part is played by the junction, remembering that a force cannot exist where it is not? Is electromotive-force a force in the Newtonian sense? Since there is no free electricity in the interior of a conductor, are the observed results to be attributed rather to free electricity associated with the conducting matter

in the heterogeneous dielectric enclosing the metallic junction? What is the part, if any, played by moisture?

Volta's procedure with such perplexities was to appeal to experiment. It is appropriate, therefore, to conclude this account of his work by directing attention to the chief results of his contact-electricity investigations in the epitomized form (Fig. 12) derived mainly from Lord Kelvin's contribution to the subject of contact force.

Apparatus, Fig. 12, i.

(1) Place D on C. Lift them together into contact with N. Break contact with N. Lift A. No divergence of G.

(2) Repeat (1), but instead of touching N with D, hold D two or three centimetres below N, with C still in contact with D. Lift A. Slight divergence of G.

(3) Perform a cycle, say 100 times, as follows: (*a*) Break contact between C and D. (*b*) Make and break contact between D and N. (*c*) Make contact between C and D. In other words, lift D up and down between N and C. Then lift A. Great divergence of G.

(4) Repeat (3), but maintain A at a distance from B. Bring a stick of rubbed sealing-wax near B. Divergence of G increases. Remove the sealing-wax. Divergence of G diminishes. Hence G has resinous (negative) electricity; *i.e.*, B has received negative electricity from the copper disc D. Interchange D and C and replace sealing-wax by glass. The charge on G is then vitreous (positive).

Apparatus, Fig. 12, ii.

(5) Varnish the polished opposed faces of D and C with shellac, and add contact-wires as in the Figure. Repeat (3) except that when D rests on C contact is to be made and broken by hand. If care is taken to keep the plates parallel, the electrification will now be greater than in the previous cases (1)—(4).

(6) In (1)—(4) examine the effect of tilting the plate D while in contact with C, at the moment of separation—the electrification can thereby be greatly reduced.

(7) Observe that experiments (1)—(4) are liable to fail if a drop of water (Fig.12. iii.) is placed on C, the lower of the two polished plates. It fails if the last connection between the zinc and the copper when D is lifted is through water.

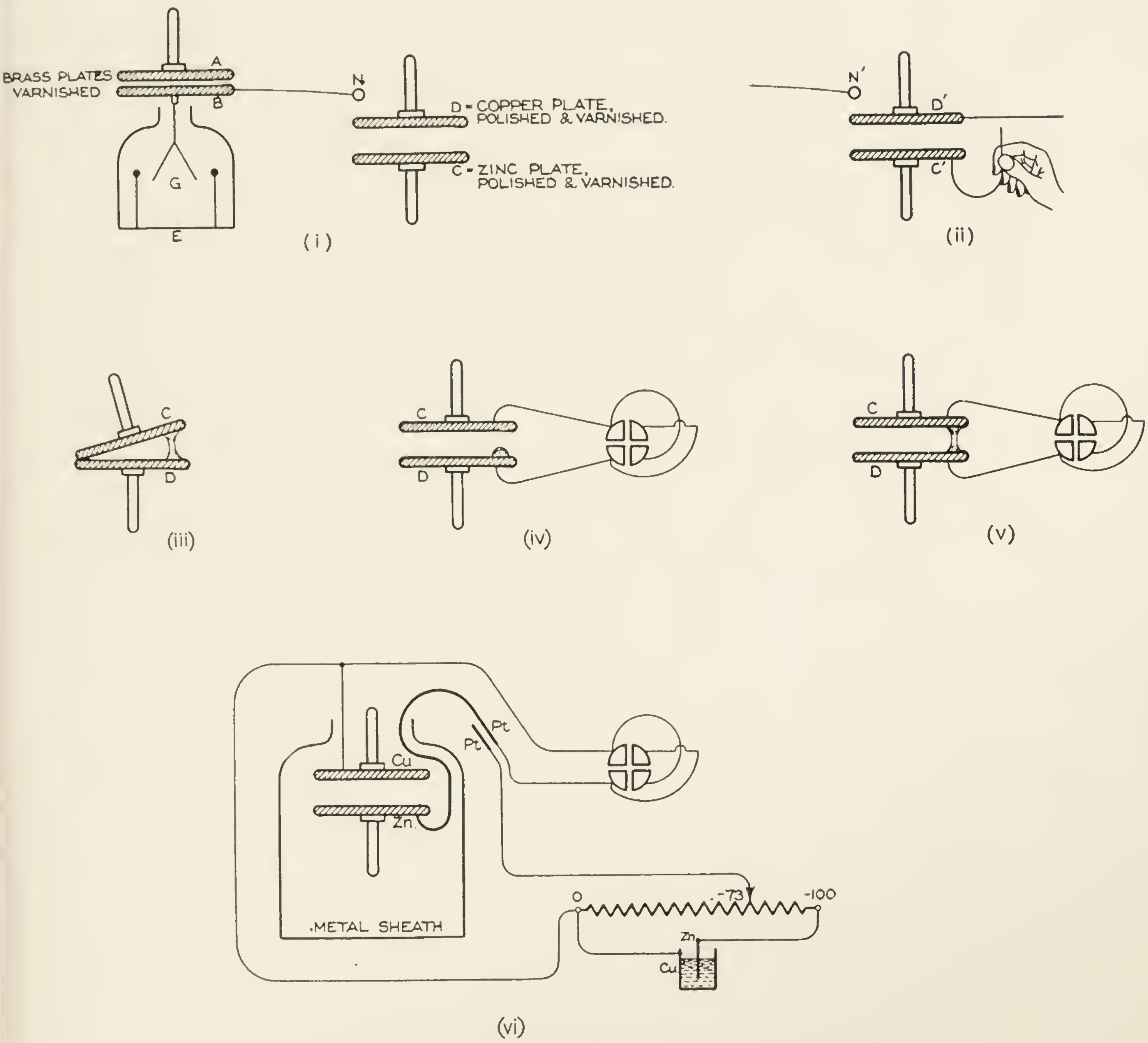


Fig. 12. Contact-Force Illustrations.

(8) In (7) if D is slightly tilted on lifting, so as to break the water arc before the separation between the metals —thus securing final contact between dry metals— the experiment does not fail, nor is it substantially altered from what is found with the dry polished metals.

Apparatus, Fig. 12, iv.

(9) Repeat (8) with an electrometer. (*a*) Let C rest on D, both being polished and dry, and note the "zero" deflection; *i.e.* the "metallic zero". (*b*) Lift C two or three millimetres above D. There is a deflection showing that vitreous (positive) electricity has passed to the insulated quadrant of the electrometer. (*c*) Bring C and D again in contact. Regain "metallic zero".

(10) (*a*) Raise the zinc disc as in (9) and place a small mound of water on D. (*b*) Bring down C to touch the top of the mound while parallel to D so as to avoid metallic contact (Fig. 12, v.). Note the deflection showing resinous (negative) electricity. (Lord Kelvin considered this motion and settlement to be "the simplest modern exhibition of Volta's greatest discovery").

(11) When the electrometer needle has settled, lift C about a millimetre above D until the water-column has broken, and then two or three centimetres further. The electrometer deflection does not alter. Lower C, there is still no motion of the electrometer needle—not even when C again touches the little mound of water.

(12) Tilt C slightly (Fig. 12, iii.) until it makes dry metallic contact with the copper, while the water remains unbroken. At the instant of metallic contact the electrometer needle suddenly leaves its deflected position and returns to the "metallic zero".

(13) Break the metallic contact between C and D. Hold C parallel to D with the water unbroken. The electrometer needle does not suddenly jump—it creeps slowly so that in a minute or so it reaches its previous steady state. (Lord Kelvin remarks that this effect was not known to Volta—it is the recovery of the Voltaic cell from electrolytic polarization).

The apparatus, illustrated in Fig. 12, vi., used by Lord Kelvin for obtaining quantitative measurements of the Volta electromotive-force is self-explanatory. Following Volta, special

attention was given by him to the nature of the surfaces of the plates; *i.e.* whether dry, polished, oxidized, clean, scratched or burnished. Kelvin concluded that the Volta force is a resultant

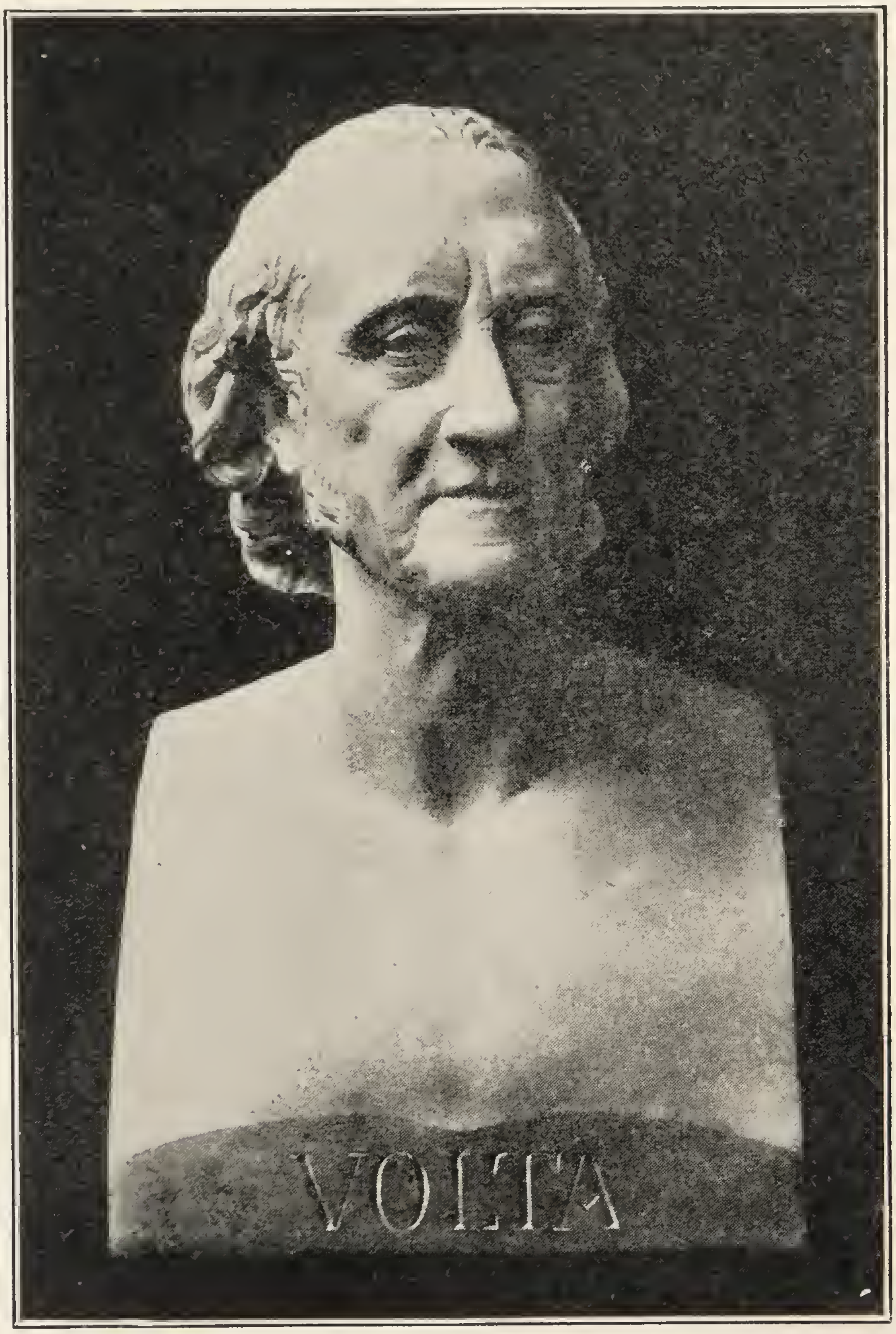


FIG. 13. BUST OF VOLTA. The original marble, by Gio Battista Comolli, was entirely destroyed. The illustration is from a plaster copy at the Reale Istituto Lombardo di Scienze e Lettre, at Milan.

of chemical affinity between thin surface-layers of the two metals. Maxwell's view was that the greater part of the force must be sought for, not at the junction of the two metals, but at one or both of the surfaces which separate the metals from

the air or other medium constituting the third element of the circuit. Heaviside suggested that on making contact between zinc and copper the first action is to remove the air from the zinc over a patch. There the disturbance begins, *i.e.* at the beginning of a zinc-copper-air line round which electric current flows. The boundary of the patch thus forms the first line of magnetic force. The work done by the zinc-air force is then equated to the sum of the electrostatic energy in the condenser formed by the system, the Joule heat in the conducting system, and the work resulting from the obscure action, if any, at the copper-air surface.

In April, 1805, Volta wrote to Humboldt stating that he was engaged upon electrometry, particularly with regard to the capacity of conductors of various forms and sizes, and the relationship of capacity to electric pressure or tension. He found that the tension diminished exactly as the capacity was augmented.

The end of the most active part of his life was approaching. In July, 1808, he was asked by the representative of a Russian University to transfer his home to that country. His reply gives an indication of what his home meant to him and his outlook upon life at that time:

> Other interests more dear to my heart, and other circumstances, oblige me to refuse. At the age of nearly 60, with two brothers ecclesiastics more aged who live with me, a wife, and three young children, I am too much attached to this family, who cherish me, and to a country that has not been ungracious. Happy in a moderate fortune and in an annual pension of 5000 francs which the Government accord me as an emeritus Professor of the University of Pavia, and as a member of the National Institute, what can I desire more for the few years that remain to me? To live in peace and in repose in my country and in the bosom of my family, to occupy myself with the education of my children without quitting my own beloved studies and my experimental researches, that is all my happiness at present, and it is for that reason that I have requested and have obtained release from the Chair of Pavia which I have held for 30 years. . . . Voila, mon respectable Professeur, ce que je dois répondre à vous et à votre ami.

Volta's biographers describe him as tall, large-browed and

noble of mien, of regular features; and, according to Arago, of country manners contracted in his youth. So far as his manners are concerned, Arago was probably wrong, for the evidence discloses a philosopher singularly gracious, fair-minded, and companionable. In 1819 Volta quitted the University and retired to Como. His relations with the world of science then ceased. In 1823 he had a slight attack of apoplexy, and on March 5, 1827, after a fever, at the age of 82, he died.

Sir Charles Wheatstone, F.R.S.

IV

CHARLES WHEATSTONE

If from a list of names of famous men associated with the early development of electrical communication we exclude those popularized in the designations of the electrical and magnetic units, the best known among the remainder is probably that of Charles Wheatstone; and if the telegraph and telephone engineers of the present generation were asked the reason for that electrician's undiminished popularity, a considerable proportion of them would attribute it, correctly enough, to "Wheatstone's Bridge". Nevertheless, he did not invent that device—on the contrary, he scrupulously assigned it to its true discoverer. Wheatstone's fame rests upon surer foundations; he was a pioneer in practical electrical communication, and a leader in the realms of qualitative and quantitative physical research. The memory of him lives—"not only for his discoveries and for the methods of investigation with which he had endowed science, but also by the recollection of his rare qualities of heart, the uprightness of his character, and the agreeable charm of his personal demeanor".

This last noble tribute from Jean-Baptiste Dumas, then Secretary of the French Academy of Science, uttered on the occasion of Wheatstone's death in Paris, scarcely more than half a century ago, has proved to be true beyond all that could have been imagined by his contemporaries. There are good reasons, therefore, for availing ourselves of any special opportunities that arise to renew or to extend acquaintance with the achievements and career of Charles Wheatstone. By the courtesy of the authorities at King's College, London, it has been possible recently to examine and to photograph some of

the relics of his apparatus, and it is proposed here briefly to recall the part such apparatus played in the establishment of the principles of observation and measurement upon which modern electrical research is founded. The "King George III. Museum" at King's College contains a collection that consists primarily of apparatus presented to the College in 1841 by Queen Victoria. It was originally brought together by George III. at the Royal Observatory, Kew. To it has been added the "Wheatstone Collection" and the "General Collection". A catalogue of the whole exists, dated 1900, but there is no detailed account available of the various pieces of apparatus in the Museum.

References to researches to which the relics here illustrated relate are to be found in innumerable volumes. If with these are included pamphlets of a controversial character, and articles in scientific and biographical works, the publications relating to Wheatstone become somewhat overwhelming. By judicious selection, however, the essential literature can be reduced to a few classic books and papers. So far as the scientific aspects of his achievements are concerned, the commemoration volume published in 1879 by the Physical Society of London, entitled, *The Scientific Papers of Sir Charles Wheatstone* supplies all that is required for general knowledge of his discoveries and of his teaching. For an account of his work, and the work of his contemporaries, relating to telegraphy, attention must be directed to two papers read before The Institution of Civil Engineers on March 2, 1852—the first by Mr. Charles Coles Adley, and the second by Mr. Frederick Richard Window. These luminous contributions to the subject are both printed in vol. xi. of the *Proceedings* of that Institution. To them must be added the obituary notice, written by one of his friends, which was printed in the *Proceedings of the Royal Society*, vol. xxiv., in 1876. In language that is at once appreciative and critical, it conveys a conception of the human side of Wheatstone, and leaves with us the impression of a man of high principle, of fine intelligence, and of invincible determination in research and its applications. What was the secret of his success? His modest early circumstances, and his immersion as a young man in commerce, could

easily have made him a thriving tradesman. But by what influences was he led along the path of research to the pinnacle of discovery?

Charles Wheatstone was born in February, 1802, in Gloucestershire. His father, a music-seller in the county town, removed with his family in 1806 to 128 Pall Mall, London, where he taught the flute, and made and sold musical instruments. Charles, who had received a private school education, showed early promise of mechanical ingenuity, and as he had clear notions of dynamical principles, he was not long in giving evidence of his capabilities. In 1821 he attracted attention by exhibiting an instrument the name and construction of which prove him to have possessed a sense of humour well calculated to dispel any priggish qualities that might have developed in such a clever youth. It was called "the enchanted lyre", and it was suspended from the ceiling by a "cord of the thickness of a goose-quill". The music appeared to proceed from a combined harp, pianoforte, and dulcimer. Wheatstone himself described it as an application of a general principle for conducting sound. A writer in the *Repository of Arts* of September, 1821, describing this instrument, foreshadowed modern broadcasting in a remarkable phrase, as follows:

> Who knows but by this means the music of an opera performed at the King's Theatre may ere long be simultaneously enjoyed at Hanover Square Rooms, the City of London Tavern, and even at the Horns Tavern at Kennington, the sound travelling, like gas, through snug conductors, from the main laboratory of harmony in the Haymarket to distant parts of the metropolis . . . perhaps words of speech may be susceptible of the same means of propagation.

It is noteworthy that this instrument was exhibited in the Adelaide Gallery, afterwards the scene of his experiments on the velocity of electricity and now part of a restaurant, to the east of the church of St. Martin's-in-the-Fields, London.

His early success with this instrument, the special features of which probably were the work of his own hands, must have been a source of great encouragement to him; for the inspirations of the physicist begin at the finger-tips, and the first

victory never loses the charm that prompts renewed effort. There is evidence also that direction was given early to his scientific work by his comprehension of the importance of the undulatory theory of light propounded by Thomas Young (1773–1829). This was the central thread of common sense upon which the pearls of analytical research were strung. His collected papers indicate how firm was his grasp of the meaning of wave-motion, and his researches show with what ease he was able thereby to transfer his ideas from acoustics to optics, and from optics to electricity. To realize the measure of his early appreciation by men of science it should be remembered that his first scientific paper was published in 1823, at a time when, with his brother, he was engaged in the manu-

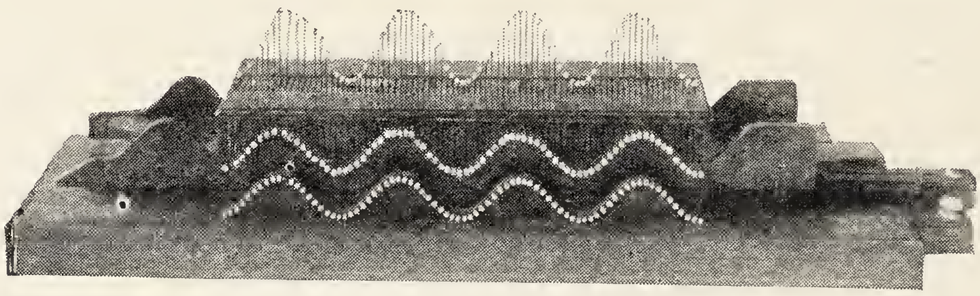


FIG. 1. WHEATSTONE'S SINE-WAVE MODEL.

facture and sale of musical instruments in London. The reward of his subsequent labours was of a kind that gave him increased facilities for extending his researches. In 1834 he was appointed Professor of Experimental Philosophy at King's College, and in January, 1836, he was elected a Fellow of the Royal Society of London.

As an indication of the general trend of his ideas, there is his memoir on Chladni's figures, and his invention about the year 1828, of the "Kaleidophone"—a simple device for combining two harmonic motions. The Kaleidophone was a steel rod of oblong section, fitted rigidly at its lower end into a heavy base-block, and provided at the top with a white bead. When displaced and suddenly released, the bead traced a curved path —determined by the respective periods and phases of the two motions of the rectangular rod—similar to the outlines obtained with a modern cathode-ray oscillograph. Fig. 1 shows his model representing wave-motion. It consists of a frame upon which

is arranged a series of bent-wire levers terminated by white beads, and operated by sliding wooden templates cut to the form of waves. The lowest curve is permanently fixed; the upper two curves are modified in phase respectively by the movement of the sliders.

Fig. 2 represents his adjustable form of Kaleidophone. It displays Wheatstone's skill in the design of mechanical gearing. The vertical rod is held near its middle point by a ball-and-

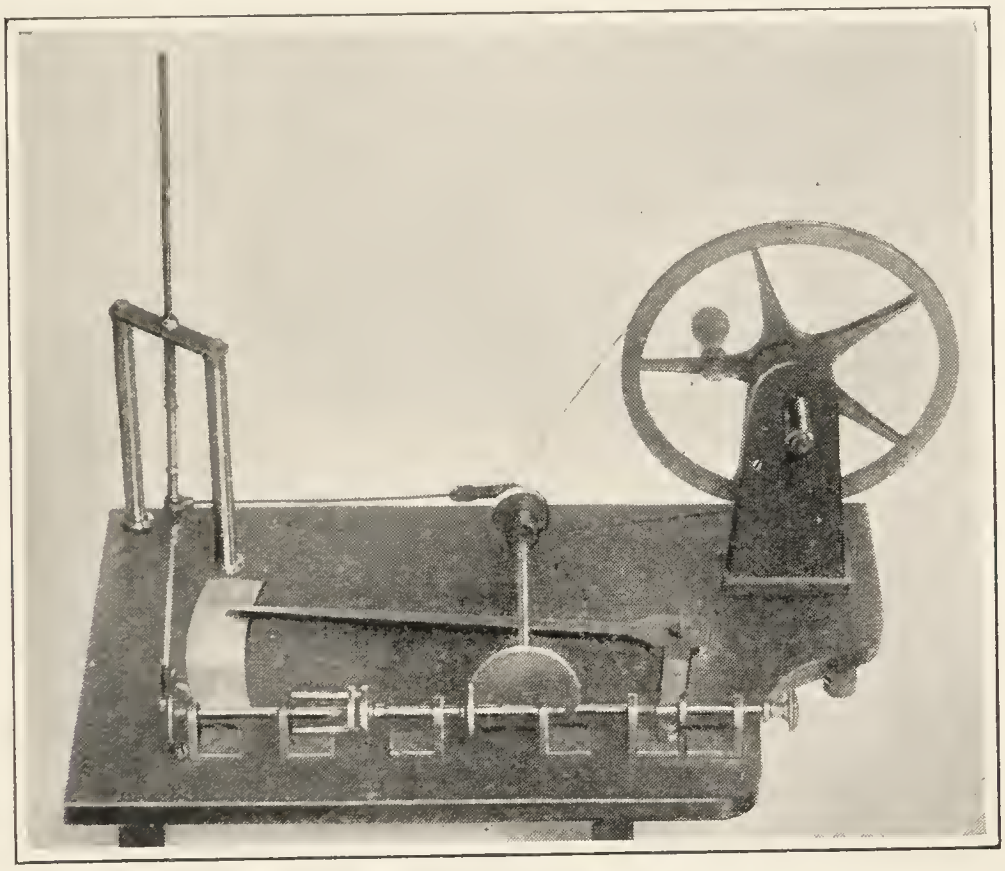


Fig. 2. Adjustable Form of Kaleidophone.

socket joint. The driving wheel causes the horizontal transverse shaft to rotate, and motion from this shaft is transferred through a friction disc to a lateral shaft. The position of the driven disc on this lateral shaft can be varied by turning a milled head at the end of that shaft, and the difference in length is taken up by a sliding clutch. Harmonic motion is thus communicated through eccentrics to the lower end of the vertical rod.

In Fig. 3 is seen Wheatstone's gas-jet organ, consisting of a group of glass tubes and gas-jets operated by a keyboard. The apparatus has deteriorated with age, but there is no doubt that

it consisted of a horizontal supply-pipe into which vertical jets were fitted, one to each glass tube, and that the glass tubes were free to move up or down. The keys, probably, were arranged to lift the pipes with respect to the jets to different heights, for "tuning" purposes. It may be supposed that this apparatus was associated with his work in 1828 with reference to resonance in air columns.

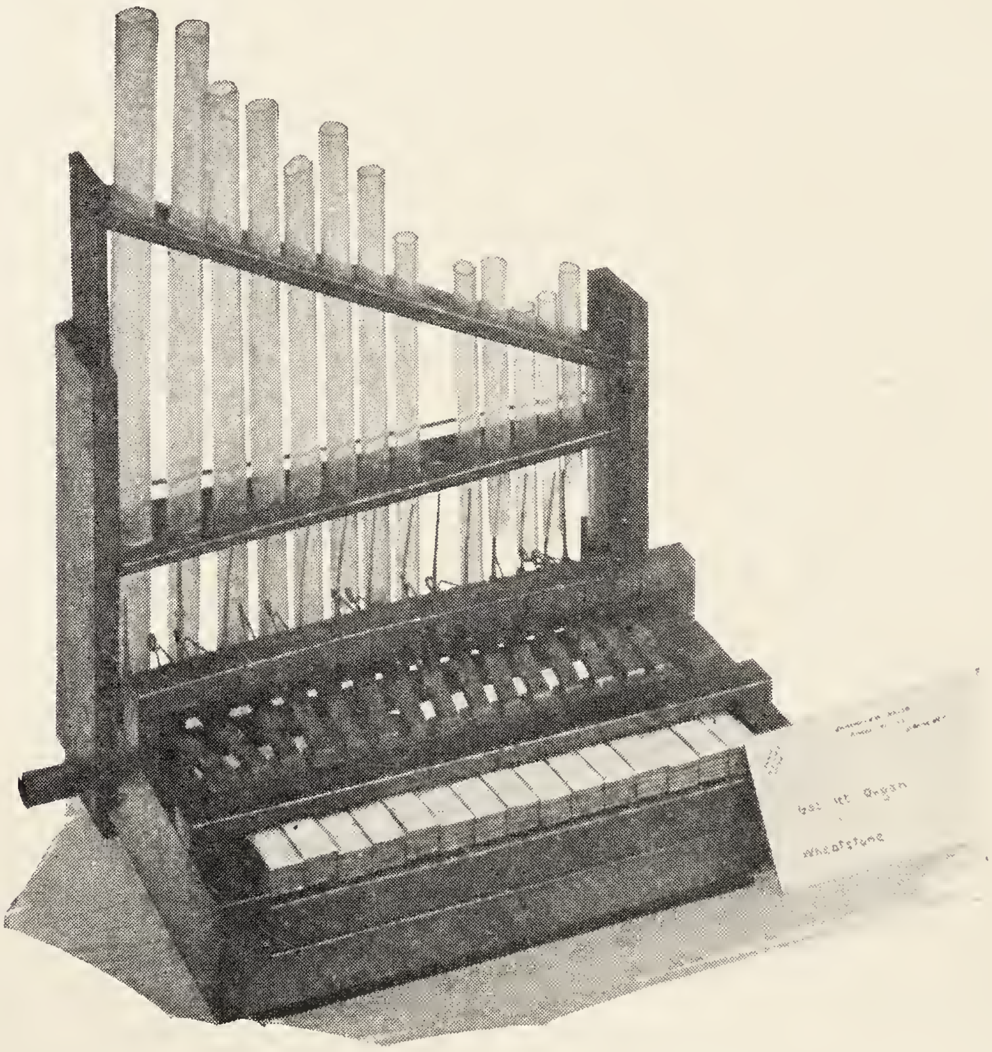


Fig. 3. Gas-jet Organ.

The next ten years of his life was a period of transition from research in acoustics to research in optics. Some of his triumphs up to this turning point in his career may be recalled by examining Figs. 4 and 5. The English concertina was invented and patented by Charles Wheatstone in 1829. The instruments to the right and left in Fig. 4 are marked "By Her Majesty's Letters Patent, Wheatstone & Co., Inventors, 20 Conduit Street, London". As Queen Victoria was not on the throne in 1829, these must not be regarded as Wheatstone's original

concertinas. The concertina-fiddle, also shown in Fig. 4, is provided with four longitudinal slots, near the bridge, one below each string, which were set into vibration after the manner of an Æolian harp. Fig. 5 is an illustration of Wheatstone's original table concertina, with foot bellows and keys for finger manipulation. Fig. 6 is his famous "Speaking Machine". This

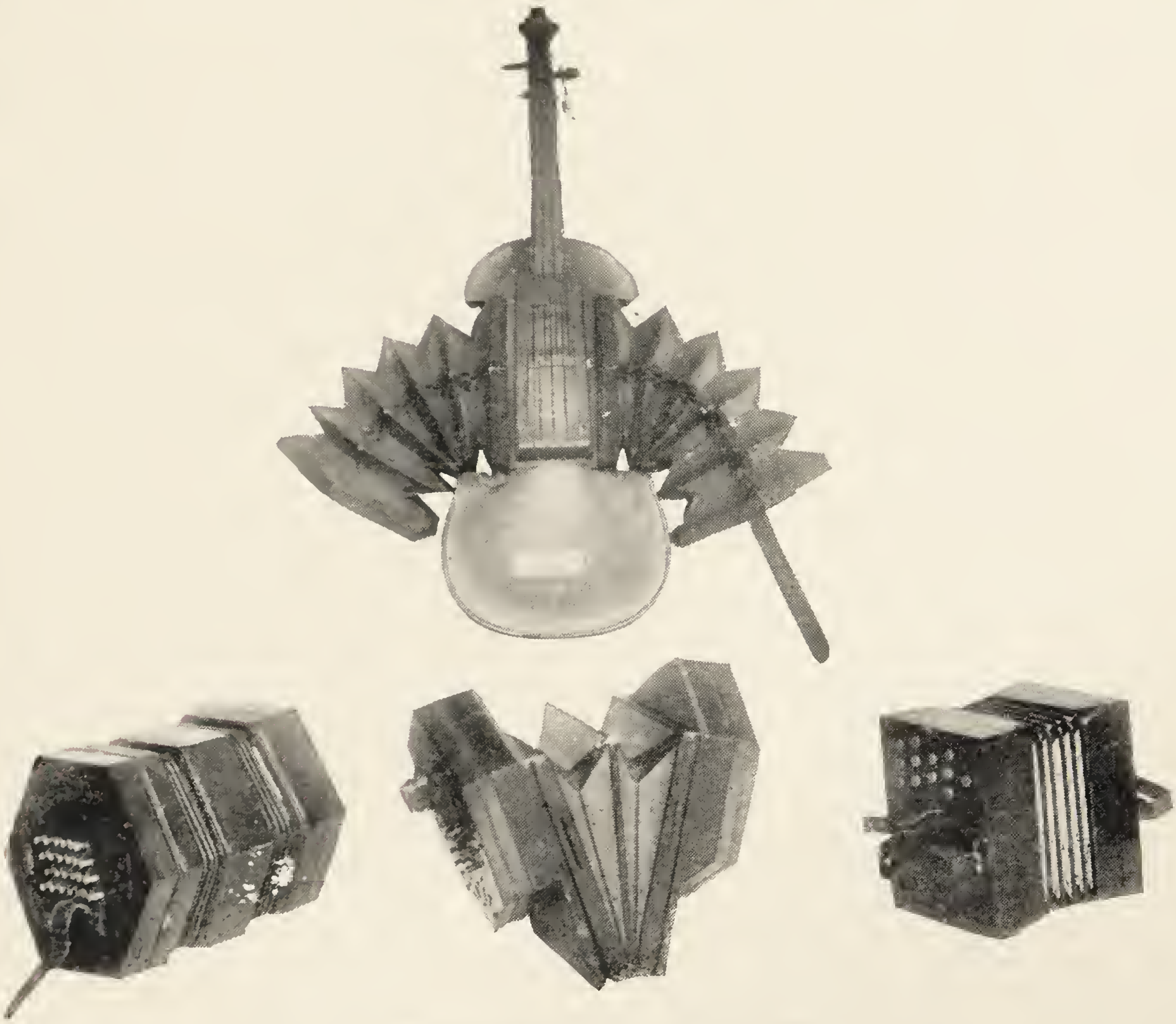


Fig. 4. Concertinas and Concertina-fiddle.

consists of hand-operated bellows on the left, and a complex resonator on the right. The right hand is placed over the trumpet-shaped orifice or "mouth", with varying degrees of movement or pressure. Above the "mouth" are seen two tubular "nostrils", and below the mouthpiece is a small yielding resonator resembling bellows. It was a modification of De Kempelen's machine (1783). This subject was dealt with by Wheatstone at the British Association in 1835 in his paper "On the Attempts which have been made to Imitate Human Speech by

Mechanical Means". He wrote also a remarkable article on the history of such devices, in the *London and Westminster Review* of October, 1837, concluding with the prediction of Sir David Brewster: "We have no doubt that before another century is completed, a talking and a singing machine will be numbered among the conquests of science."

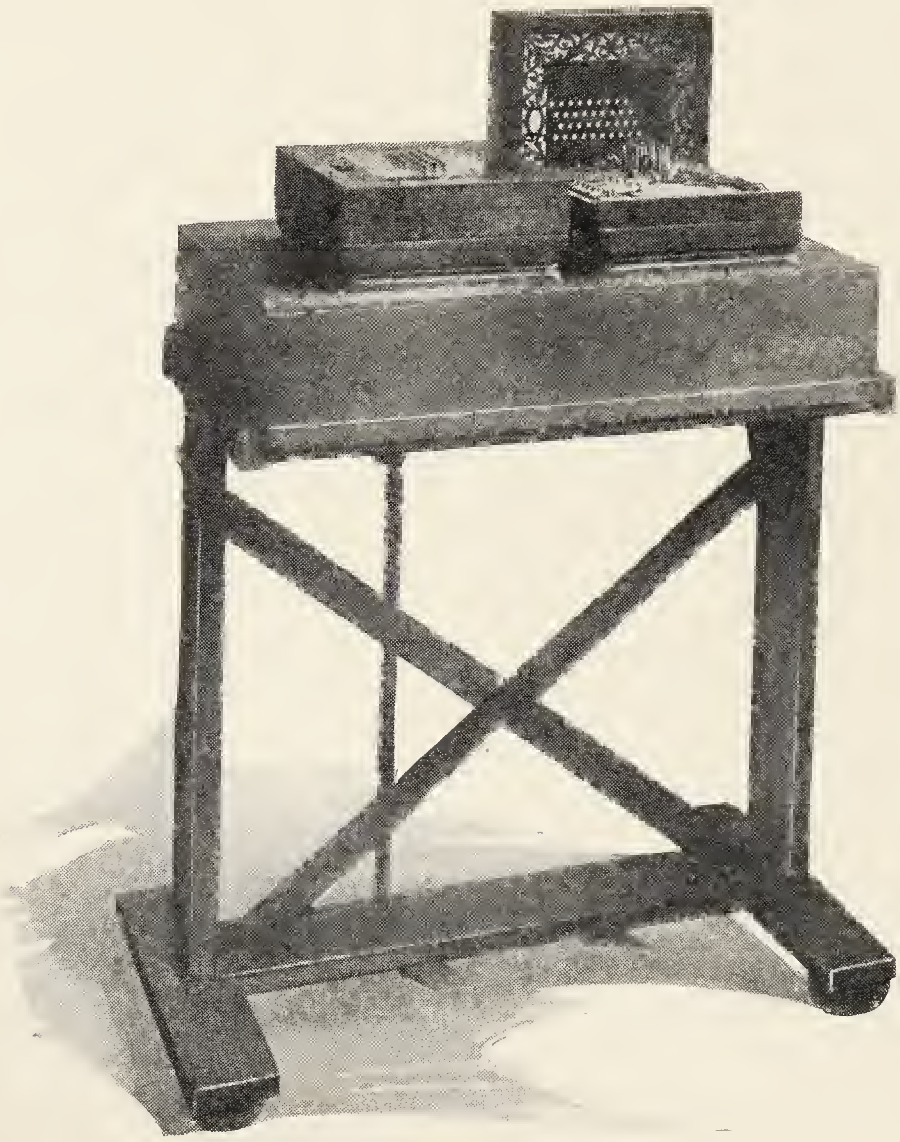


FIG. 5. TABLE CONCERTINA.

If there were no other record of his genius as a research worker than his paper written in 1835 on "The Prismatic Analysis of Electric Light", his fame would have been perpetuated; for he there announced the existence of bright lines in the spectrum emitted by the incandescent vapour of metals volatilized by the heat of an electric discharge—a mode of discriminating metallic bodies more readily than that of chemical examination. Thus he laid the foundations of spectrum analysis and was an early worker at emission phenomena.

Fig. 7 is an example of Wheatstone's polar clock. It depends for its operation upon a discovery by Sir David Brewster that the plane of polarization of the sky is always 90 degrees from the sun. The instrument contains a double-image prism and a thin plate of selenite enclosed in a tube placed parallel to the earth's axis. When the prism—which carries an index traversing a circular arc marked with the hours—is turned round until no colour is perceived, the index points to the time of day.

In 1838 he wrote on binocular vision and produced the reflecting stereoscope, embodying the principle that the notion of solidity in vision depends upon the mental superposition of two pictures of the same object in dissimilar perspectives.

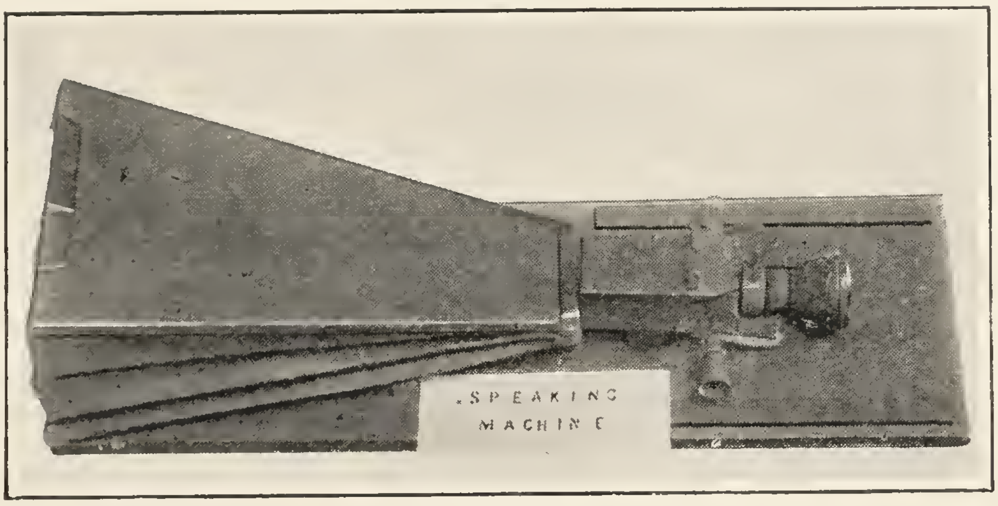


Fig. 6. Speaking Machine.

Brewster subsequently used for this purpose the wedge-shaped segments of large lenses, in which the lens and prism arrangement due also to Wheatstone were combined. In 1858 Wheatstone extended this research, and thereby wove the threads of his early achievements in acoustics and optics into the fabric of his later success in telegraphy. Wheatstone was knighted in 1868, following upon the success of his automatic telegraph.

"Wheatstone's Bridge" was invented by Samuel Hunter Christie (1784–1865). In his Bakerian Lecture (1843) Wheatstone described it as "The Differential Resistance Measurer", and he leaves no doubt for posterity to resolve concerning its origin. He says: "Mr. Christie in his 'Experimental Determination of the Laws of Magneto-Electric Induction' printed in the *Philosophical Transactions of the Royal Society for* 1833, has

described a differential arrangement of which the principle is the same as that on which the instruments described in this section have been devised. To Mr. Christie must, therefore, be

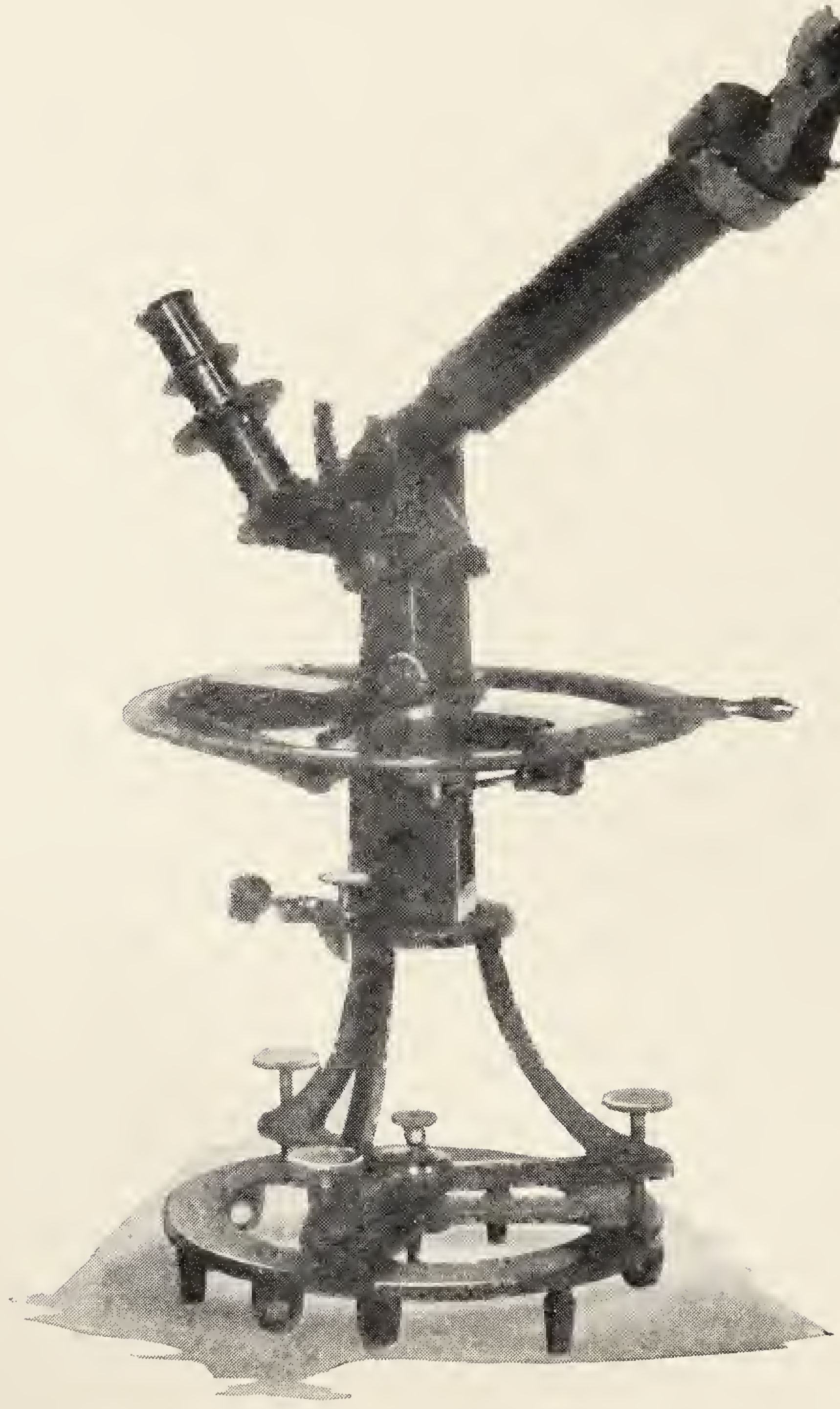


FIG. 7. POLAR CLOCK.

attributed the first idea of this useful and accurate method of measuring resistances."

Fig. 8, which illustrates the original in the King's College Museum, is self-explanatory, except for the small lever attachment fitted to the upper middle terminal. This was used for making a fine adjustment of what we should now call the

"variable arm". For this purpose the lever was swung round to left or right until it made contact with one or other of the wires of the two arms shown at the top of the illustration of the bridge, and the rotation was continued until balance was obtained.

In the introduction to his Bakerian Lecture, Wheatstone stated that the instruments and processes he was about to describe were all founded on the principles established by Ohm

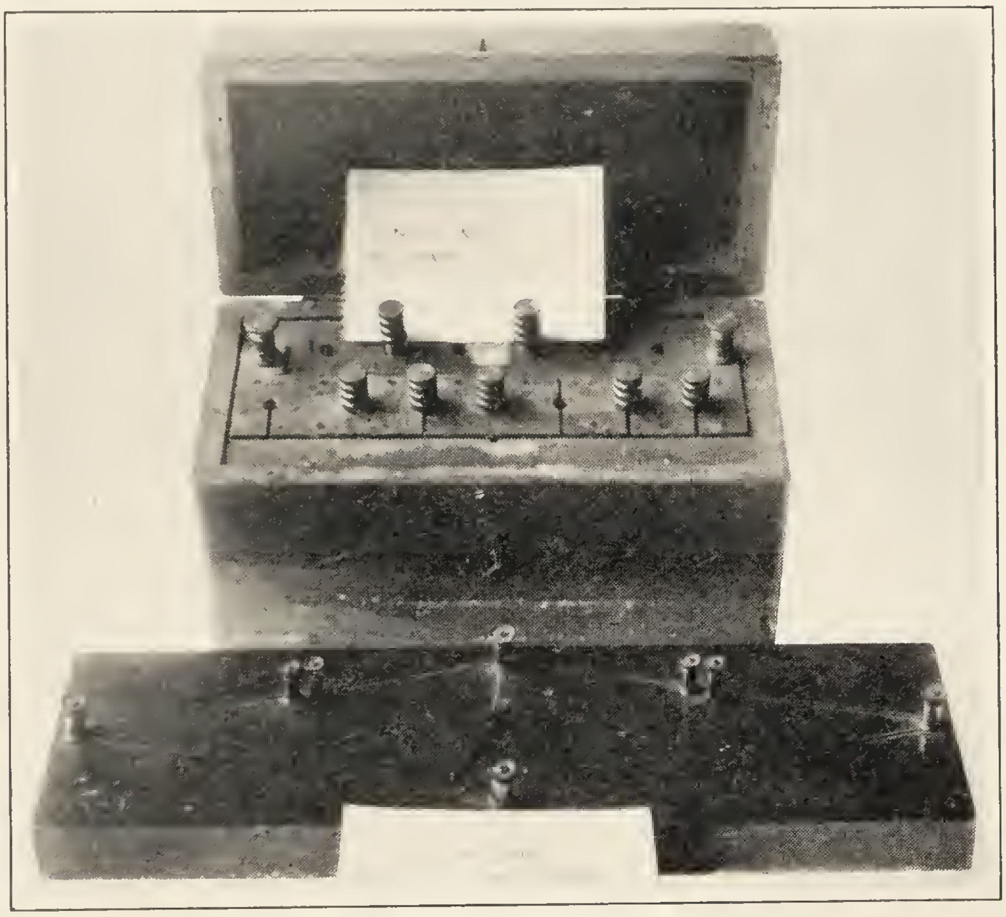


FIG. 8. WHEATSTONE'S BRIDGE AND RESISTANCE BOX. The box is marked "Miles" near the first plug-hole on the left front.

"not yet generally understood and admitted even by many persons engaged in original research". He proceeded to show the need for a correct standard of resistance; he adopted for his unit the resistance of a copper wire one foot in length and weighing 100 grains, and he stated the diameter as 0.071 of an inch. One of the original resistance boxes in the King's College Collection (Fig. 8) is marked in "Miles"—thus adumbrating the Mile of Standard Cable. At the same time, he gave an account of "the differential galvanometer proposed by M. Becquerel". This, which in a later generation became an instrument of pre-

cision, in Wheatstone's day presented constructional difficulties. It is sufficient here to note its comparative antiquity, its supersession in 1843 by the bridge, and the association of the differential-galvanometer principle and the bridge principle as alternatives in the development of duplex telegraphy by Gintl (1853), Stearns, and others.

Wheatstone's generous and unqualified ascription to Christie of what to-day would be termed the "bridge principle" is more creditable and precious than any self-seeking claim could have been. The relics here illustrated serve to remind us how rapidly Christie's idea, Ohm's law, and Wheatstone's genius, conspired to produce a practical "bridge". To see the matter in true perspective, it is only necessary to turn to the original communication by Christie in the *Philosophical Transactions of the Royal Society*, vol. 123, 1833. He there describes an investigation to confirm what in modern language would be called the law of change of resistance with length, material, and cross-sectional area of wires, which had been clearly stated by Ohm six years earlier. He used two forms of apparatus.

In the first form, Fig. 9, two wires of equal length and of different material, usually copper and iron, were wound differentially; *i.e.* in reversed directions, respectively, upon an iron core. The ends of the dissimilar wires were joined, and were connected to a galvanometer. The core was then placed across the poles of a large magnet. When the core was suddenly removed, the deflection, if any, of the galvanometer was observed.

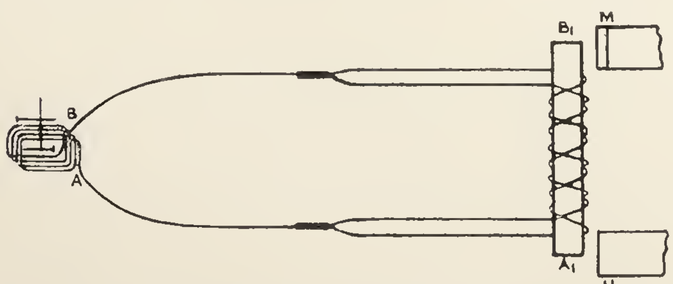


FIG. 9. CHRISTIE'S DIFFERENTIAL DOUBLE HELIX RESISTANCE BALANCE.

In the second form the arrangement was as represented in Fig. 10. Here dissimilar wires were again connected in pairs, CC^1, CD^1, and DC^1, DD^1. In Christie's own words:

On the contact of the ends of the iron cylinder with the poles of the magnet being made or broken, a current of a certain intensity being excited in the helix round the iron cylinder, became, at the

points C^1D^1, the source of currents in the copper and iron wires; at the points CD, equal facilities were afforded by the wires CB, DA, for the transmission of these opposing currents to the galvanometer, where consequently, their difference might be very accurately measured. Or viewing the subject in a somewhat different light, at the points C^1D^1, two routes are presented to the current excited in the wire of the helix, one through the copper wires, the other through the iron, and the effect at the galvanometer would measure the difference in the conductivity powers of the two metals.

Again, he states:

When I first made use of the arrangement which I have described, the subject being quite new to me, I was not aware of that employed by M. Becquerel. There is some similarity in the two, but the principles on which their application depends are very differ-

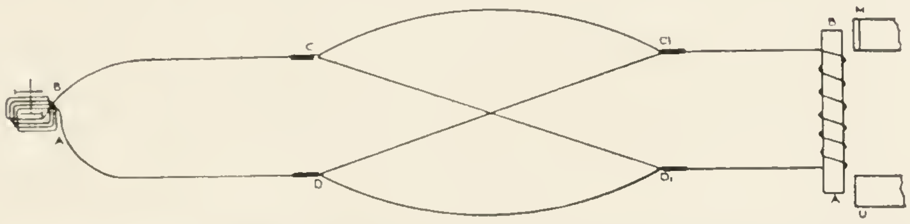


FIG. 10. CHRISTIE'S SINGLE HELIX, FOUR-WIRE RESISTANCE BALANCE.

ent. M. Becquerel's depends upon two equal currents, in separate wires, being equally diminished by two other currents, likewise in separate wires: mine, on the effect of a current in a single wire being counteracted by an equal and opposite current in the same wire, or that the opposite electricities neutralize each other, so that no current exists in the wire of the galvanometer. It appears to me that my arrangement combines the advantages of greater simplicity and greater accuracy.

The dynamical method of computation adopted by Christie should be examined in detail; it illustrates the difficulties encountered by those who, in Wheatstone's phrase, had "not yet generally understood" Ohm's law, in dealing with network problems.

Wheatstone's investigations of the "Velocity of Electricity and the Duration of Electric Light" are described in the *Philosophical Transactions of the Royal Society of* 1834. Examples of his original pieces of apparatus with their revolving mirror, from the King's College Collection, are illustrated in

Figs. 11 and 12. The story of his preliminary failures and of his constant determination to overcome all obstacles in this research must be read in detail to be appreciated. Some of his experiments on the time occupied by sparks to pass through insulated wire were carried out at "the Gallery in Adelaide Street". The greatest elongation he observed of the projected image of the spark was 24 degrees, corresponding to 1/24000 second, and for the velocity through the wire he obtained "288,000 miles in a second". In this trial the mirror rotated

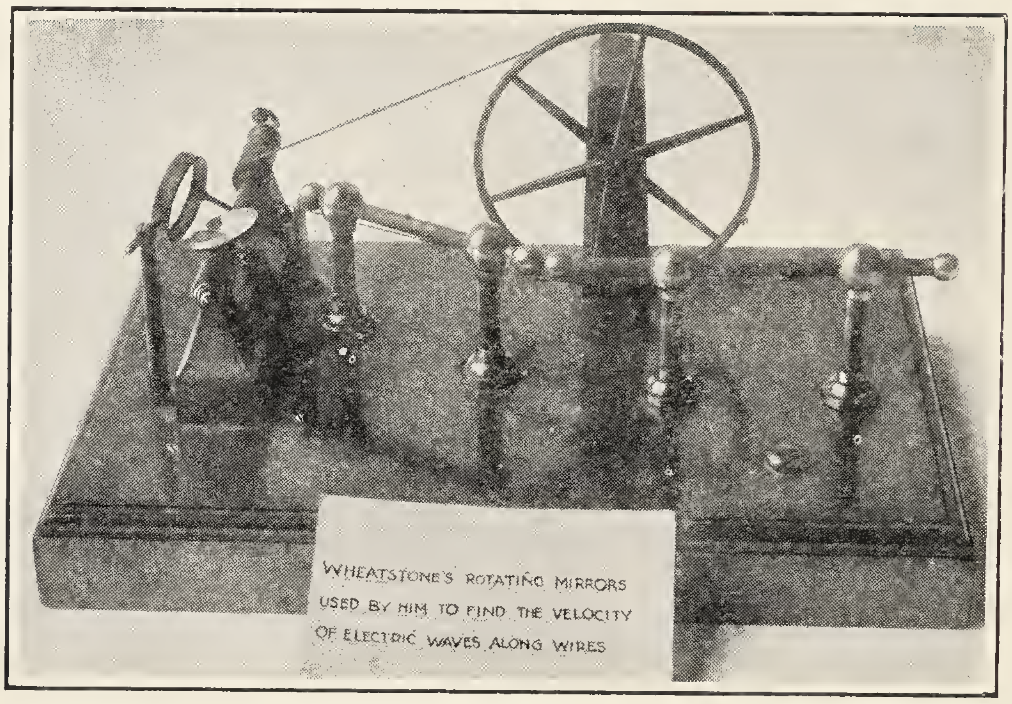


FIG. 11. SPARK-GAP AND ROTATING MIRROR.

800 times in a second. He also investigated the rate at which an electric wave travels through a wire, by suspending half a mile of copper wire in the vaults under King's College. Three interruptions of the circuit were made at three pairs of brass knobs. He repeated this research with four miles of wire.

With reference to these experiments Oliver Heaviside long afterwards pointed out (*Electrical Papers*, Part II., p. 395) that Wheatstone's result: "has not been supported by any later results, which are always less than the speed of light (even in a distortionless circuit). But a reference to Wheatstone's paper on the subject will show, first, that there was confessedly a good deal of guesswork; and next, that the repeated doubling of the wire on itself made the experiment, from a modern point

of view, of too complex a theory to be examined in detail, and unsuitable as a test."

There is not space here to recount the wonderful story of Wheatstone's share in the development of telegraphy. The lamentable dispute with his partner, William Fothergill Cooke, will exemplify to all time the need for definite agreements between the principals in such enterprises, and the deplorable

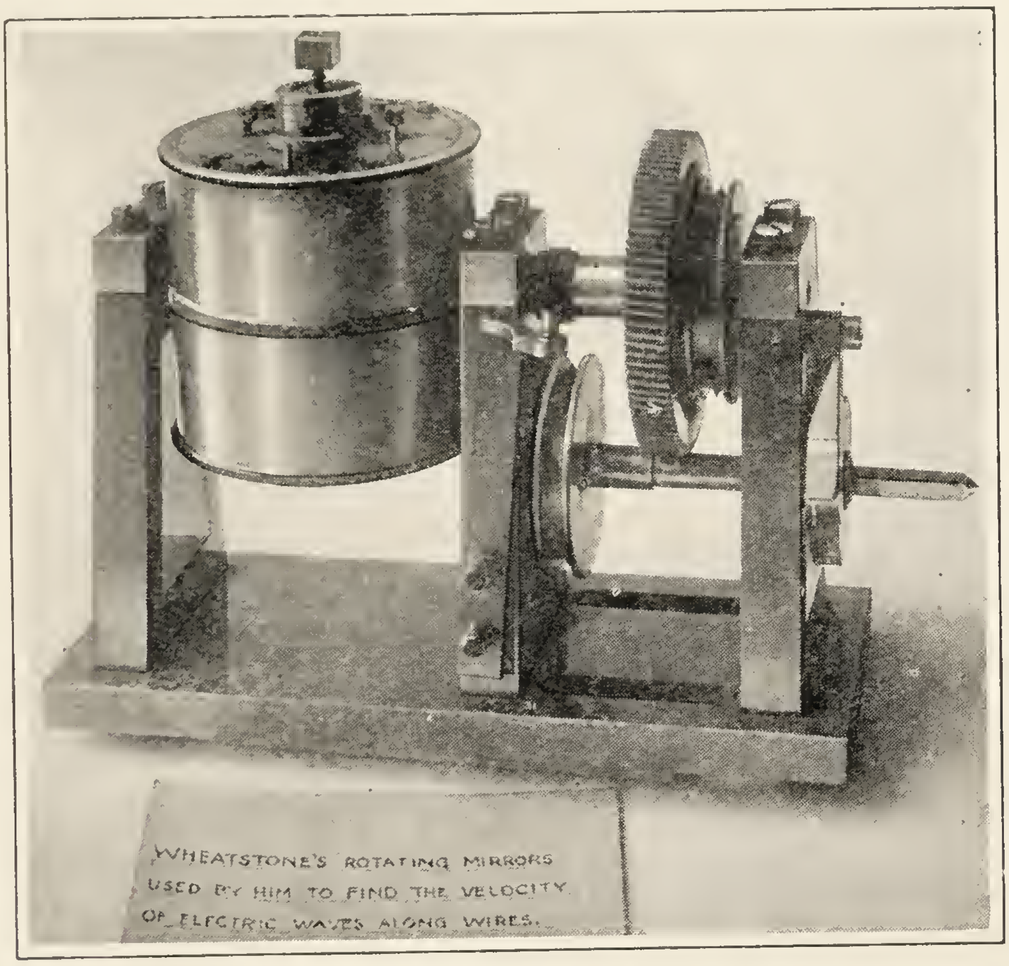


FIG. 12. ROTATING MIRROR.

waste of energy and time and the destruction of happiness that result from personal friction. It must suffice to state with regard to the crowning achievement that Wheatstone's contemporary, De la Rive, said, "the philosopher who was the first to contribute by his labours, as ingenious as they were persevering, in giving electric telegraphy the practical character that it now possesses is undoubtedly Mr. Wheatstone". Of the combined efforts of the partners, it was declared by the late Willoughby Smith on the occasion of the Extraordinary General Meeting of the Society of Telegraph Engineers and of Electricians held in Paris during the Exposition Internationale

d'Electricité, September 21, 1881, that "no account of a practical electric telegraph had been published prior to the date of Messrs. Cooke's and Wheatstone's patent of June, 1837".

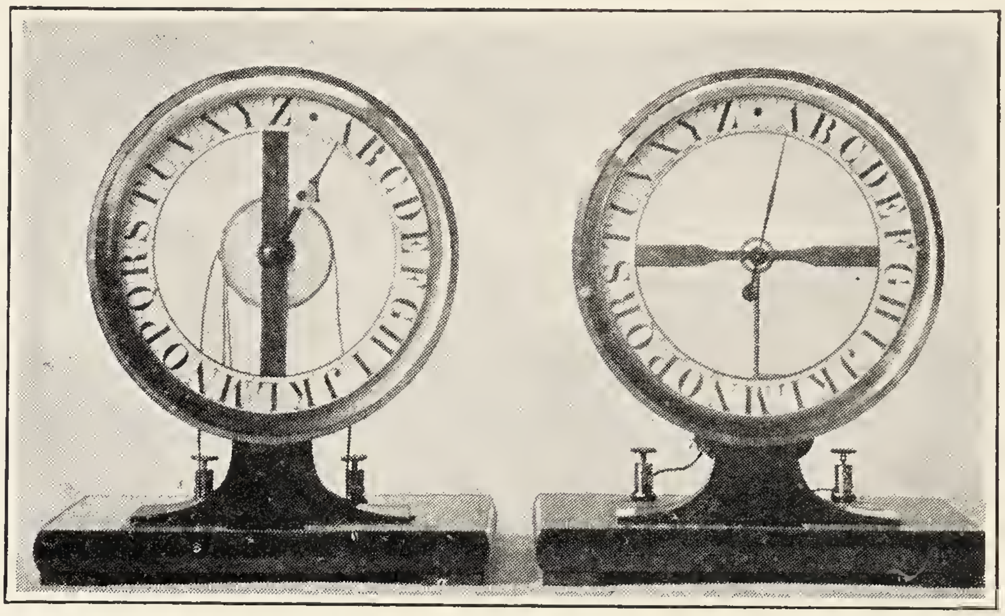


FIG. 13A. "LETTER-SHOWING TELEGRAPH" (Front).

Among the illustrations of Wheatstone's apparatus may be seen the original of his Letter-showing instrument (Figs. 13A and 13B). The maker was Ruhmkorff, Paris. Fig. 14 shows

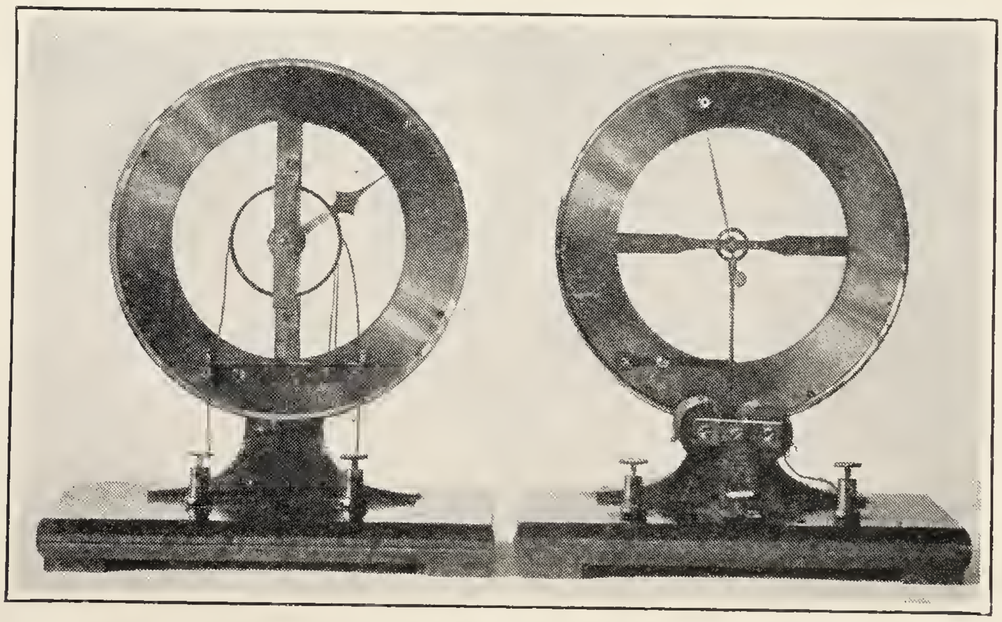


FIG. 13B. "LETTER-SHOWING TELEGRAPH" (Back).

Wheatstone's Relay. A V-shaped piece of metal attached to a magnetic needle is brought—when the needle is deflected—into contact with two mercury surfaces in a divided insulating cup. Fig. 15 depicts Wheatstone's tape puncher; Fig. 16 Wheat-

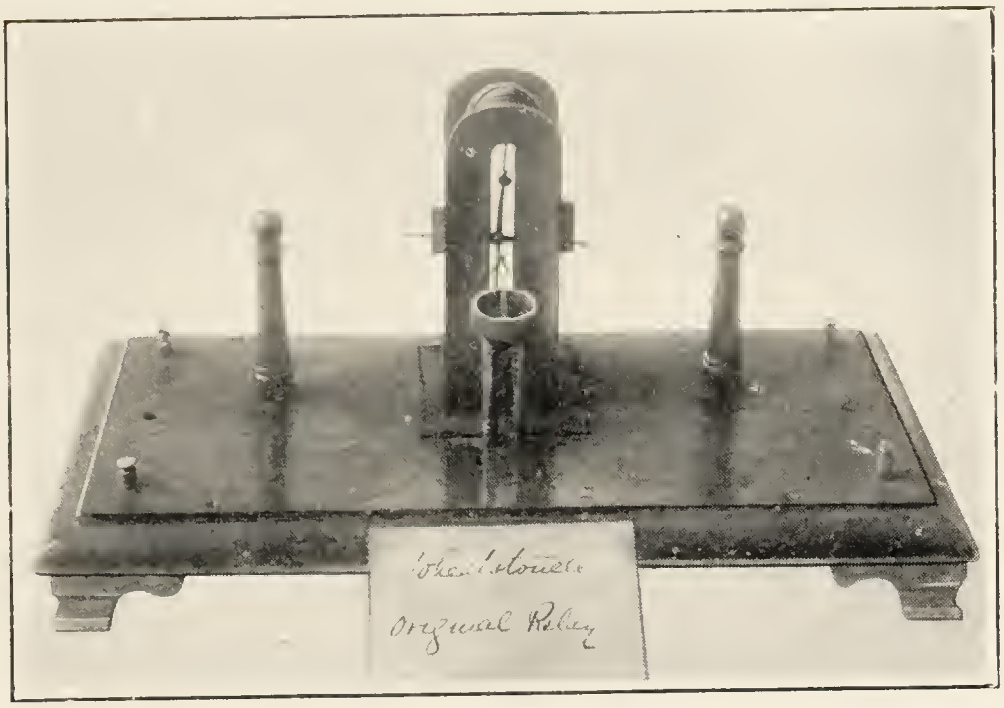


FIG. 14. ORIGINAL RELAY.

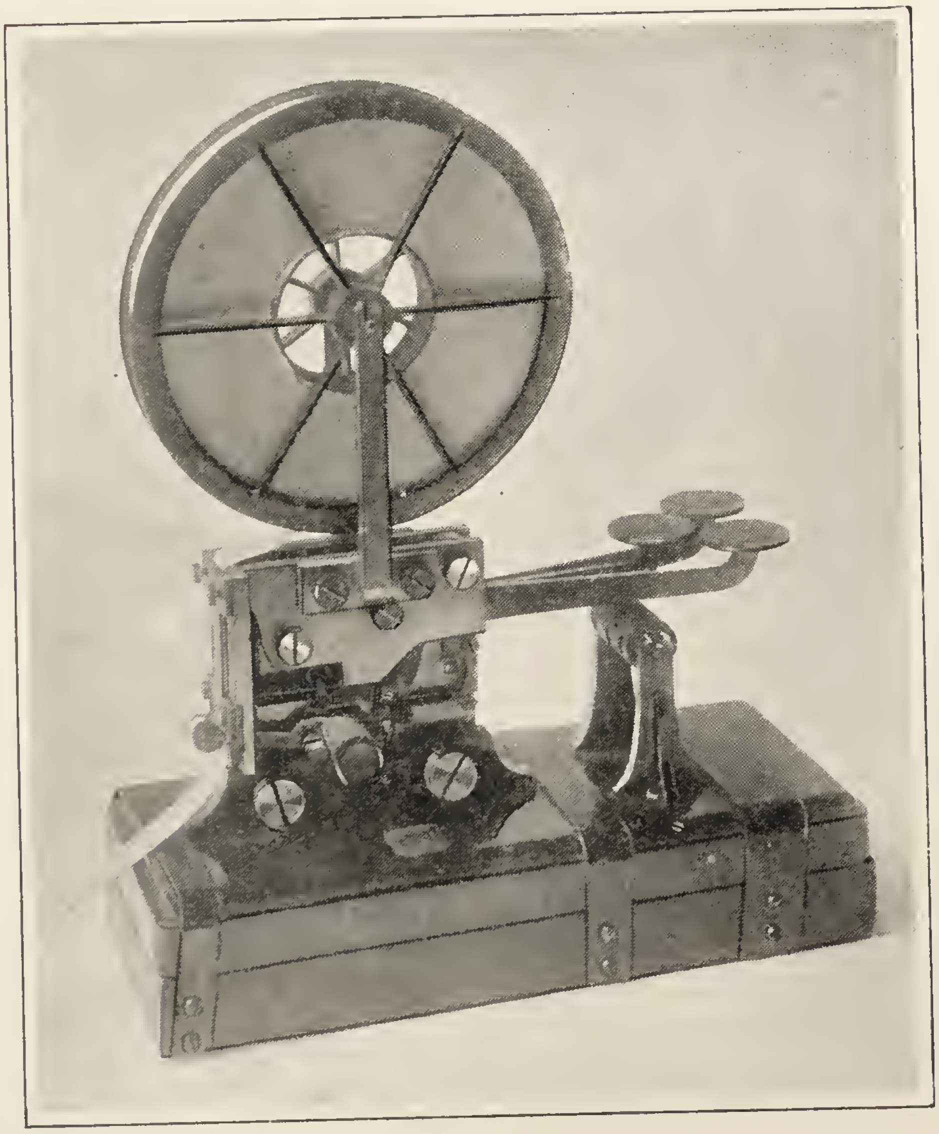


FIG. 15. TAPE PUNCHER.

stone's five-needle telegraph; and Fig. 17 Wheatstone's single-needle telegraph "sender" and "receiver".

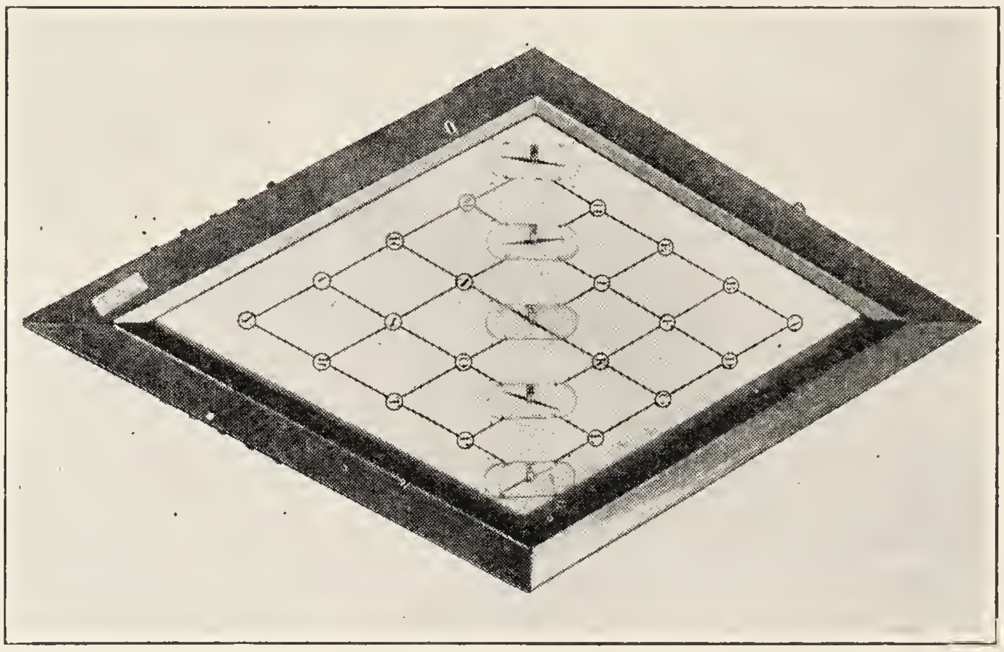


Fig. 16. Five-needle Telegraph.

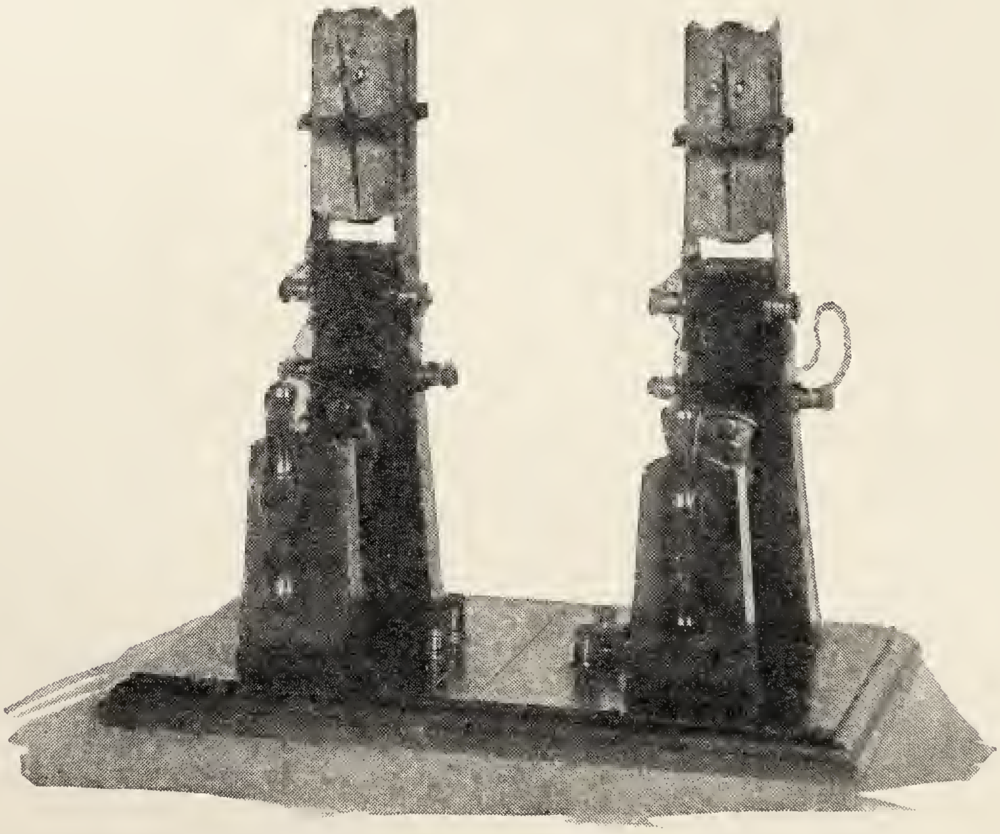


Fig. 17. Single-needle Telegraph "Sender" and "Receiver".

The tale of the five-needle electric telegraph (1837) is well told by Professor J. A. Fleming (*Fifty Years of Electricity*). This telegraph was being worked between Fenchurch Street

and Blackwall railway station, when three of the five dials broke down. The telegraph clerks, however, made up a code for working with the remaining two and the result was quite as good, if not better, than before. Thereafter one needle was found sufficient.

Fig. 18 reminds us that Wheatstone contributed to the development of the dynamo. S. P. Thompson in his treatise on the dynamo has indicated the main features of Wheatstone's

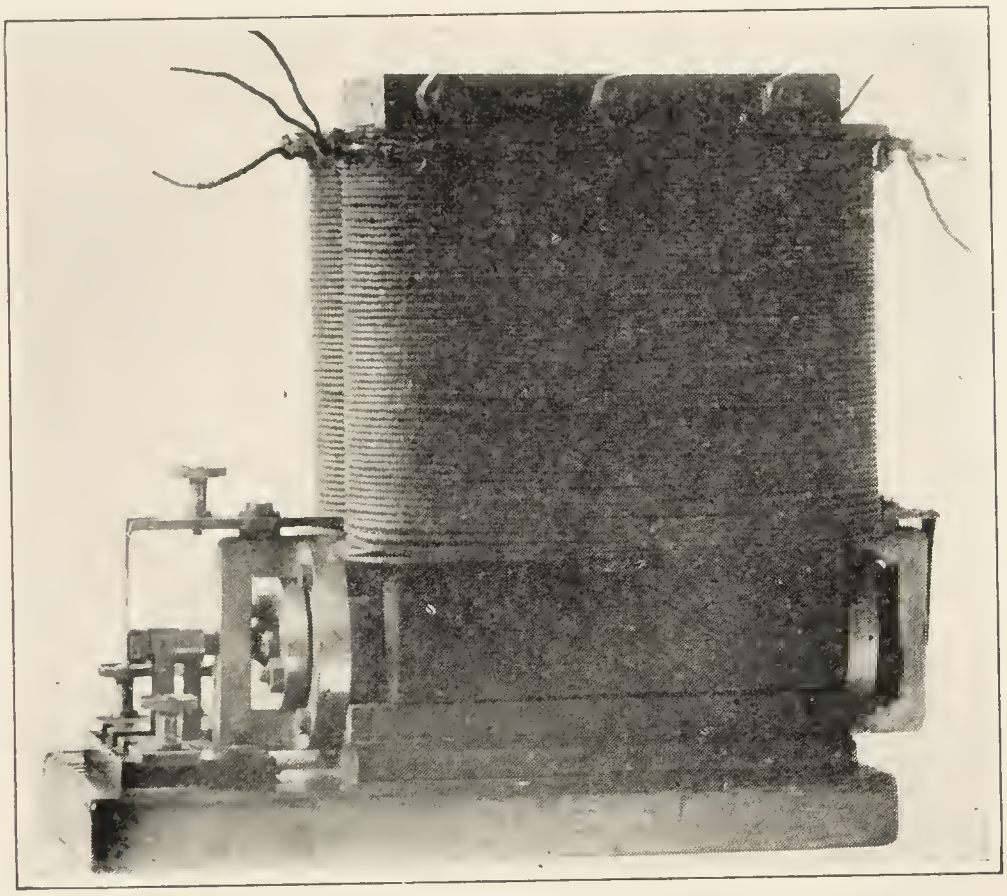


FIG. 18. WHEATSTONE'S DYNAMO.

part in that work. Wheatstone began his improvements in 1841, with a machine in which for the first time the armature coils were so grouped as to give a really continuous current.

In 1856, C. W. Siemens took out a provisional patent for the shuttle-wound longitudinal armature, invented by Dr. Werner Siemens. On January 17, 1867, Dr. Werner Siemens described a self-exciting dynamo in which the exciting coils were in the main circuit in series with the armature coil. On February 14, 1867, Wheatstone described to the Royal Society his invention of a similar machine in which the exciting coils were connected as a shunt. A self-exciting machine without permanent mag-

nets had been constructed for Wheatstone by Stroh in the summer of 1866. In 1867 Ladd exhibited a self-exciting machine having two shuttle-wound armatures—a small one to excite the common field magnet, a large one to supply currents for electric light.

It is fitting that there should be found with Wheatstone's apparatus a tribute to the work of Henry. The precise history of the coils of copper strip, insulated with silk (Fig. 19) cannot be ascertained, but the label, which has been attached to them for some years, declares them to have been used by Henry in

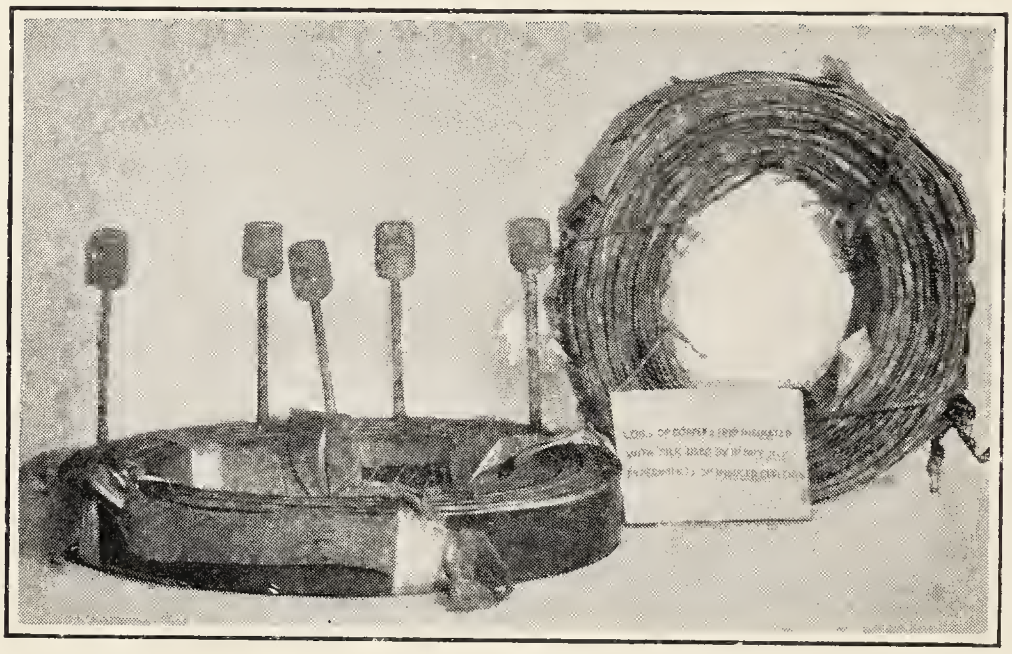


FIG. 19. COILS OF COPPER STRIP, INSULATED WITH SILK, used by Henry in his experiments on induced currents.

his experiments on induced currents. Henry visited England in February, 1837, and met Wheatstone, Faraday, and Daniell at King's College. The three philosophers there exchanged ideas, and carried out experiments together. It is possible that these coils were used by Henry in this demonstration; but whatever their origin they recall a fellowship that made history, and a meeting which Henry in subsequent years remembered with pleasure.

The relics include two photographs, one of Wheatstone, reproduced in the frontispiece, and the other (Fig. 20) of Wheatstone in a group with his friends: Faraday, Huxley, Brewster and Tyndall. In Fig. 20 Wheatstone is holding a Morse Key, while his companions are examining his Inkwriter, which is

upon the table. The cell at the side of it appears to be a "Bunsen", but it may have been a "Daniell". It is appropriate that Wheatstone should be there amongst his peers: Faraday, the prince of experimenters; Huxley (1825–1895), the would-be

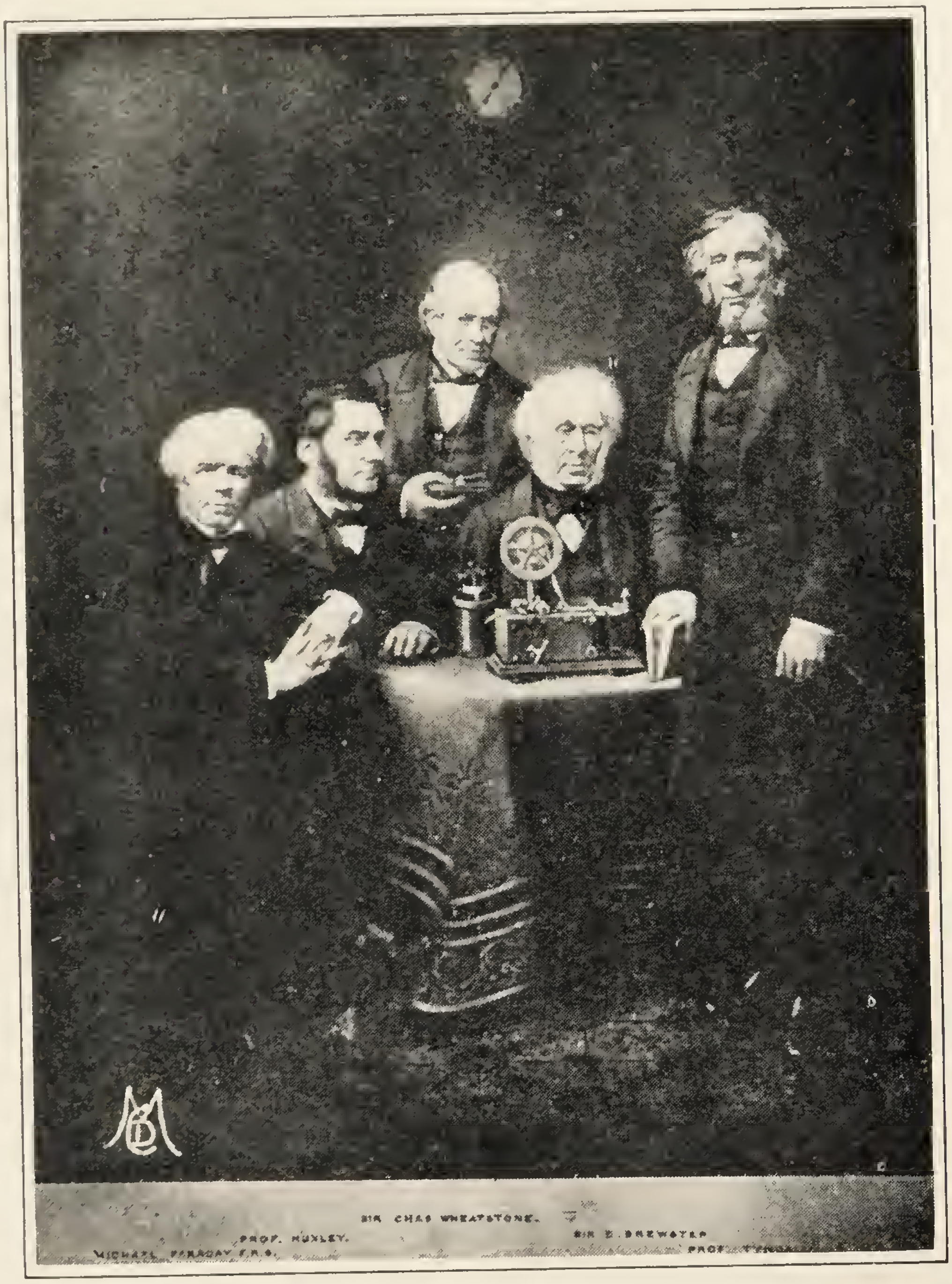


FIG. 20. MICHAEL FARADAY, PROFESSOR HUXLEY, SIR CHARLES WHEATSTONE, SIR DAVID BREWSTER, AND PROFESSOR TYNDALL.

engineer who became a leading biologist and the most astute controversialist against the dogma of his day; Brewster (1781–1868), the poet, preacher, physicist, the inventor of the Kaleidoscope, the biographer of Newton, and the writer of three hundred and fifteen papers on scientific subjects; and Tyndall (1820–1893), the Irish National-schoolboy who became

an engineer, a student in Germany, a professor of natural philosophy at the Royal Institution, a good sportsman, a colleague of Faraday, a physicist of the first rank, who made the Alps his own, and a writer unexcelled in the whole range of scientific literature. With the exception of Brewster, none of the group received what may be called systematic education, all were ardent research workers, all became Fellows of the Royal Society, all were distinguished writers. A man is known by his friends, and by these may be known Charles Wheatstone.

Portrait of Heinrich Hertz.

V

HEINRICH RUDOLF HERTZ

The work of the earliest pioneers of electrical communication culminated in the spring of 1886, when Heinrich Rudolf Hertz demonstrated at Karlsruhe the wave character of electrical transmission through space and through wires. With self-detachment as worthy of remembrance as the discovery itself, he hastened to suggest that he had but verified what others had foretold, that some phenomena exhibiting electric waves had been observed earlier by von Bezold, and that the theory upon which the Karlsruhe results were based was derived from Faraday, Maxwell and von Helmholtz. Physicists throughout the world at once recognised, however, that where others had hesitated Hertz had gone forward with firm steps, that where others had surmised he had measured, and that where others had comprehended in part he had established the theory of a universal group of converging facts. To all experimenters his announcement brought encouragement and inspiration. It was the old injunction, *Fiat lux.*

Heinrich Rudolf Hertz was born at Hamburg on February 22, 1857. He was the eldest son of the advocate, Gustav Hertz. On his father's side he was of Jewish origin. His mother, whose maiden name was Elizabeth Pfefferkorn, was of Frankfort-on-Main where her father was a doctor of medicine, descended from a family that for generations had been Lutheran preachers in south Germany. The Hertz family had long resided as merchants in Hamburg. The grandfather of Heinrich was successful in that business and became a townsman of considerable local importance. This ancestor, as a pastime, studied natural science, and made for himself a small laboratory, the apparatus from

which came into the hands of Heinrich as a boy. There is still treasured by Frau Professor Hertz at Bonn a chemical balance that belonged to that precious collection.

The father of Heinrich practised at Hamburg, Lubeck, and Bremen. He was selected from the jurists to be Oberlandes-

MOTHER OF HEINRICH HERTZ.

gerichtsrat and afterwards to be a Senator of Hamburg. For him also natural science had attractions, but his principal study was language. Even when he attained the age of eighty-six and his eyes were no longer able to discern the text, he employed a scholar to read to him from the classics. This delight in language was transmitted to his son. Heinrich was an eloquent master of his own tongue; he was familiar with English and

French, he knew enough Italian to read Dante with pleasure, and he retained intimate knowledge of the humanities. It is said that, as a youth in Hamburg, he purchased from a bookstall an Arabian grammar, he decided to master it, and he engaged a teacher—one Redlob—to instruct him in Arabic.

FATHER OF HEINRICH HERTZ.

Thus the great physicist was born of dual race, into a family that had risen in the scale, and that by nature was intent upon intellectual studies. While he was a lad, there moved over Europe the trade-winds of technical education, with a pressure centre in Germany. Under this influence the relative prospects of "techniker" and "klassiker" were freely discussed, and choice had to be made between a commercial and a professional career.

It is remarkable that, notwithstanding these circumstances, the education of Hertz was throughout unorthodox, and to some extent self-chosen. Rudiments were acquired by him at the private Bürgerschule of Dr. Lange, in Hamburg, where boys were prepared for city avocations as practical townsmen, with-

Heinrich Hertz (1886).

out Greek or Latin. Hertz was quick to discern that his friends at other schools derived advantage from the classics, and he persuaded his father to arrange for him to have special lessons in those subjects. Accordingly, he left the Bürgerschule at fifteen, and had a private tutor every day for one hour. During the remainder of each day, he studied by himself, and it was at this period that he fitted out a room at home with bench and

lathe to make simple apparatus for experiments in physics and chemistry.

At seventeen he entered the Gelehrtenschule of the Johanneums at Hamburg, where he completed his classical studies. He left there at Easter, 1875, with the diploma. It was at this stage that he realized the distinction between engineering and pure science. He doubted whether he was adapted for scientific work; he was conscious of the pleasure to be derived from problems of mechanical construction, and he decided to study for a "practical year" with a firm of engineers at Frankfort-on-Main. At nineteen he proceeded to the Technical High School of Dresden, where he remained for six months. Then followed a year of military service with the Eisenbahn-Bataillon of Berlin. Once more he had to choose between the same two careers, but at last—in November, 1877—he realized that only in natural science could he find the freedom, the scope and the field of discovery for which he longed. He went to Munich, nominally for engineering studies, "surveying, building construction, builders' materials, and such like"; actually, however, after consulting his parents and obtaining their approval, he diverted his course towards mathematical and experimental physics—the territory he was destined to make his own. That winter was spent in seclusion for the study of mathematics, mechanics, and physical laboratory work. A year later he was transferred to Berlin to acquire the stride of the giants—von Helmholtz and Kirchhoff.

His first independent research in physics had its origin in a question set as a prize subject by the philosophical faculty of the University of Berlin:

> If electricity moves with inertia in bodies, then this must, under certain circumstances, manifest itself in the magnitude of the extra-current—*i.e.* in the secondary current which is produced when an electric current starts or stops. Experiments on the magnitude of the extra-current have to be made, such that a conclusion can be drawn from them concerning inertia of the electricity in motion.

It will be recalled that a somewhat similar question had presented itself to Maxwell. In the endeavour to solve the riddle,

Hertz carried out at the University a series of investigations, using some of the apparatus from his home. He wrote the account of his results during a period of military service, at Freiburg, and he gained the prize—a gold medal. The research in itself was of little importance, for the results were negative in character. Its value was in revealing the philosopher and in giving him a bent and encouragement upon the threshold of his career.

The speed at which he reasoned and worked on this investigation, on his subsequent researches, on induction in rotating spheres, and on the distribution of electricity over the surface of moving conductors, was astonishing. It can only be accounted for by the zeal and delight with which he entered into the game of scientific discovery. His merit was at once recognized by the Berlin Philosophical Faculty. The University of Berlin conferred upon him the distinction of doctor and the rare award, *magna cum laude.*

His paper on induction in rotating spheres—the subject of his inaugural dissertation when taking his degree—reveals how early he became acquainted with the theorems of Maxwell, for it bears the date of March 15, 1880.

In October, 1880, Hertz was selected by von Helmholtz as demonstrator in physics. This association lasted until Easter, 1883. There is a legend that when von Helmholtz, at this period was asked to explain any obscure points in electrical theory, he would advise his interrogators to consult his assistant, adding that "Dr. Hertz has already arrived at the answers to these questions". Two centuries earlier a relationship somewhat of like kind existed between Galileo and Torricelli.

From Berlin, Hertz departed in 1883 to take up the appointment of lecturer in theoretical physics at the University of Kiel. There he devoted himself partly to the solution of theoretical problems, and partly to the clearing up of doubtful questions by experiment. What the future held in store for him was indicated by definite foreshadowings. On January 27, 1884, he wrote in his diary: "Thought about electromagnetic rays. Reflected on the electromagnetic theory of light"; and in May of that year there are the entries: "Hard at Maxwellian electro-

magnetics in the evening. Nothing but electromagnetics. Hit upon the solution of the electromagnetic problem this morning."

His colleagues at Kiel recognized his qualities. They expected that his skill in experimental demonstration would lead him to things of importance. In two respects he quickly justified their conjectures. At Easter, 1885, he became professor in ordinary of experimental physics at the Technische Hochschule at Karlsruhe; and in 1886, at the age of twenty-nine, he married Elizabeth Doll, the daughter of Dr. Doll who was teacher of surveying at Karlsruhe University. The home of Hertz became a happy centre of university life. The laboratory at the Hochschule provided him with the means of materializing his ideas in electromagnetics.

In the spring of 1886, he found amongst the apparatus stored in the small but well-equipped preparation room at Karlsruhe, "a pair of so-called Riess or Knochenhauer spirals"—flat coils insulated with sealing-wax (Fig. 1)—and he observed that the discharge of even a small Leyden jar through one of these sufficed to cause a spark to pass across a short air-gap between the ends of the other. It was the smallness of the jar needed for this purpose that first attracted his attention. Then he observed that, in a neighbouring conductor, side-sparks were produced. With a special form of sparking device he investigated the matter, and he discovered along this neighbouring conductor a neutral point. He thus recognized that he was dealing with oscillatory discharges, and he concluded that the series of oscillations in the conductor was regular.

By the courtesy of the administrators of the Deutsches Museum of Munich, it is possible here to illustrate some of the original apparatus used by Hertz in those early experiments. The collection was acquired partly from Frau Elizabeth Professor Hertz, and partly from the Technische Hochschule of Karlsruhe. To realize its significance, it is necessary to consider where the work of Hertz enters the pages of electrical history. It was pointed out by Oliver Heaviside, in the *Philosophical Magazine* of March, 1877, that the oscillatory nature of a condenser discharge in association with self-induction was first discovered by Joseph Henry in 1842, and that this work had

been somewhat overshadowed by the discoveries made by Faraday. The theory of the reaction between a condenser and coil was given by Sir William Thomson—afterwards Lord Kelvin—in the *Philosophical Magazine* of June, 1853. The effect of self-induction in association with capacity in a telegraph line

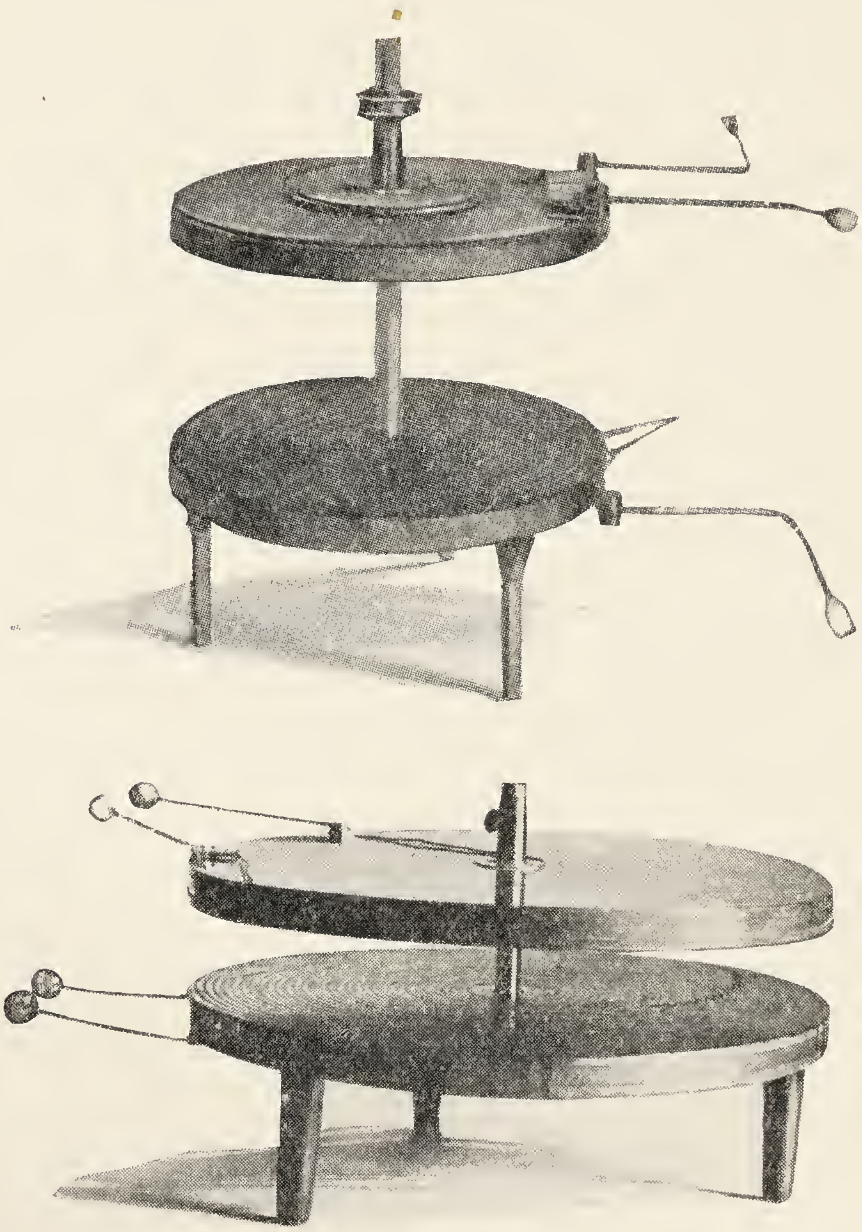


FIG. 1. THE KNOCHENHAUER SPIRALS USED BY HERTZ AT KARLSRUHE.

was first considered by Kirchhoff, in 1857, on the basis of the electrodynamic theory propounded by William Weber. Von Helmholtz dealt with the problem of the oscillations of a Leyden jar in 1847. It was worked at by Fedderson in 1858, and in the years 1861–62 he demonstrated the phenomenon with revolving mirrors. Fedderson's original apparatus for this purpose is at Munich; it includes the negative he obtained of the light-band corresponding to the fluctuations of illumination at

a frequency in agreement with Kelvin's formula connecting periodic time with self-induction and capacity. In 1864, Maxwell predicted that electromagnetic waves, in free space, would have the velocity of light. Upon this basis, the oscillations observed for example in the particular cases examined by Fedderson would have corresponded to wave lengths of about 30 metres—lengths so great in comparison with the dimensions of the laboratories, that the wave character of the propagation in free space escaped notice by physicists preceding Hertz.

Next in order of time came the results of W. von Bezold. These were described in the *Berichte der Bayrischen Akad. d. Wissensch.* in 1870, and were summed up by von Bezold himself as follows:

1. If, after springing across a spark-gap, an electric discharge has before it two paths to earth, one short and the other long, and separated by a test-plate, the discharge current splits up, so long as the sparking distance is small. But when the sparking distance is larger, the electricity rushes only along the shorter path, carrying with it, out of the other branch, electricity of the same sign.
2. If a series of electric waves is sent along a wire which is insulated at the end, the waves are reflected at the end, and the phenomena that accompany this process in the case of alternating discharges appear to be caused by interference between the advancing and reflected waves.
3. An electric discharge traverses wires of equal lengths in equal times, whatever may be the material of which these wires consist.

The early experiments of Hertz were carried out without knowledge of what had been accomplished by von Bezold. When the attention of Hertz was drawn to those results he paid a graceful tribute to them, and ultimately in his treatise upon electric waves he reproduced von Bezold's paper in a place of honour as an early chapter.

Hertz knew that the time of oscillation of small quantities

of electricity in a circuit such as he was using is determined by an equation involving the resistance, self-inductance, and capacity of the circuit, including the spark-gap, and he proceeded to vary these cardinal factors to obtain quantitative results. He quickly realized that, by appropriate adjustment, a condition of "harmony", *i.e.* resonance, could be brought about between oscillating conductors and neighbouring circuits. In Fig. 2, the upper part illustrates the method of operating the

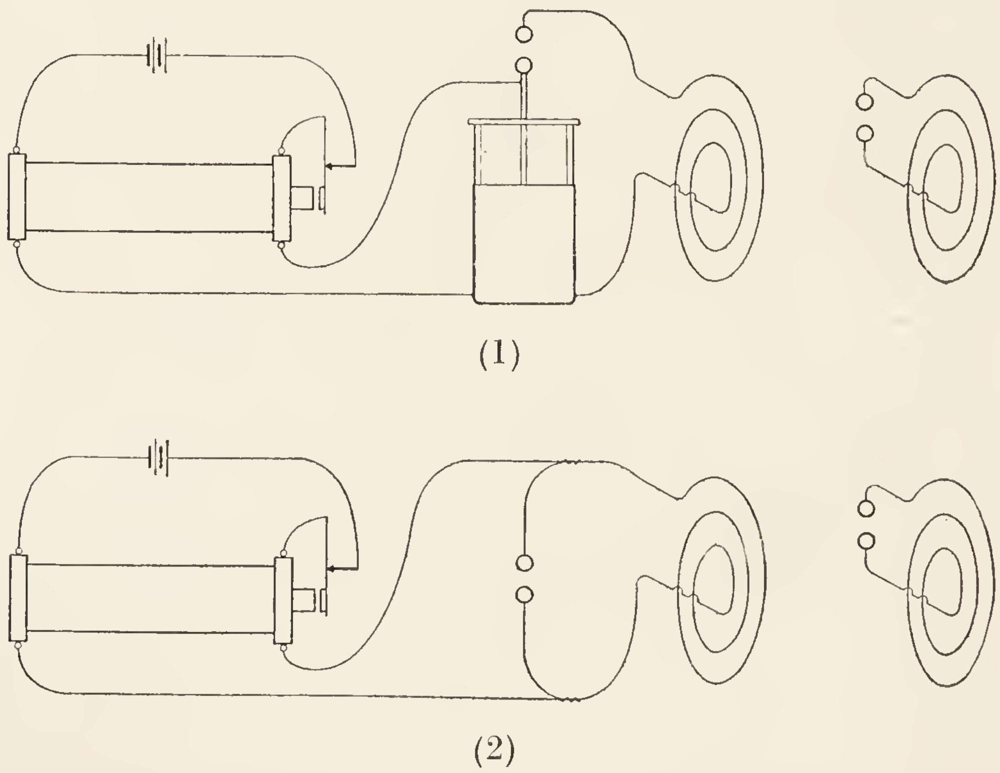


FIG. 2. ARRANGEMENT OF KNOCHENHAUER SPIRALS: (1) With Leyden jar charged by induction coil; (2) Without Leyden jar, so as to obtain higher frequency by reduction of capacity.

Knochenhauer spirals, where as was usual before Hertz, a Leyden jar is discharged through a circuit with no pretence to resonance. To increase the frequency, Hertz sought to reduce the capacity; he therefore eliminated the Leyden jar, as indicated in the lower part of Fig. 2. By removing the Leyden jar, the two circuits became automatically resonant with one another. The primary circuit in this second arrangment was energized directly from the induction coil. By this means he produced electric waves. But he was not only concerned with the production of waves. He was intent upon investigating the nature of the "polarization", if any, that the theories of Fara-

day and of Maxwell require in the medium between the generating and resonant circuits. At his hands, therefore, the dielectric received special attention.

The next development was to replace the generating spiral

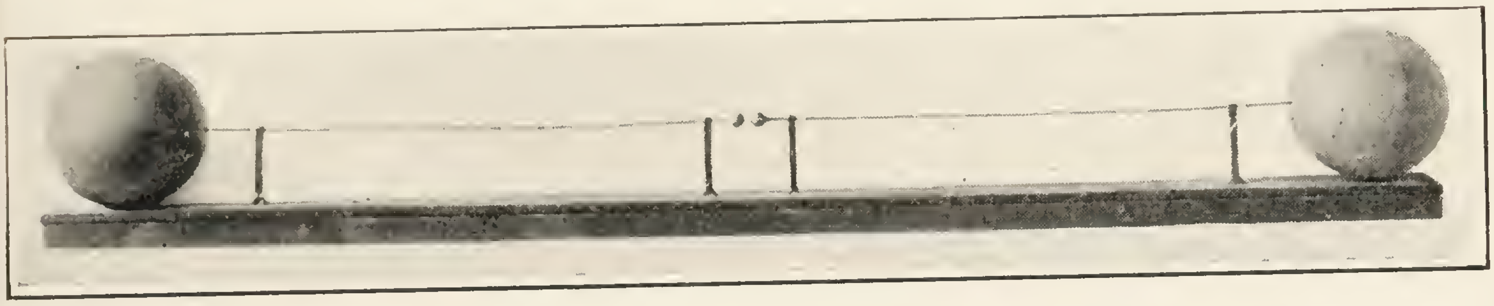


FIG. 3. THE FIRST OSCILLATOR OF HERTZ. Two copper wires, each 1 metre in length, supported on rods of sealing-wax. The large spheres are of sheet zinc, and are 30 centimetres in diameter. Base 260 × 7·5 centimetres.

by a straight conductor, Fig. 3, containing a central spark-gap. To adjust the capacity, the outer ends of the rods forming the conductor could be fitted with metal spheres or plates. This apparatus, when connected to an induction coil as in Fig. 2, became the Hertz oscillator. The original apparatus had a frequency of about a hundred million oscillations a second. He found that the receiver also might be a straight conductor of

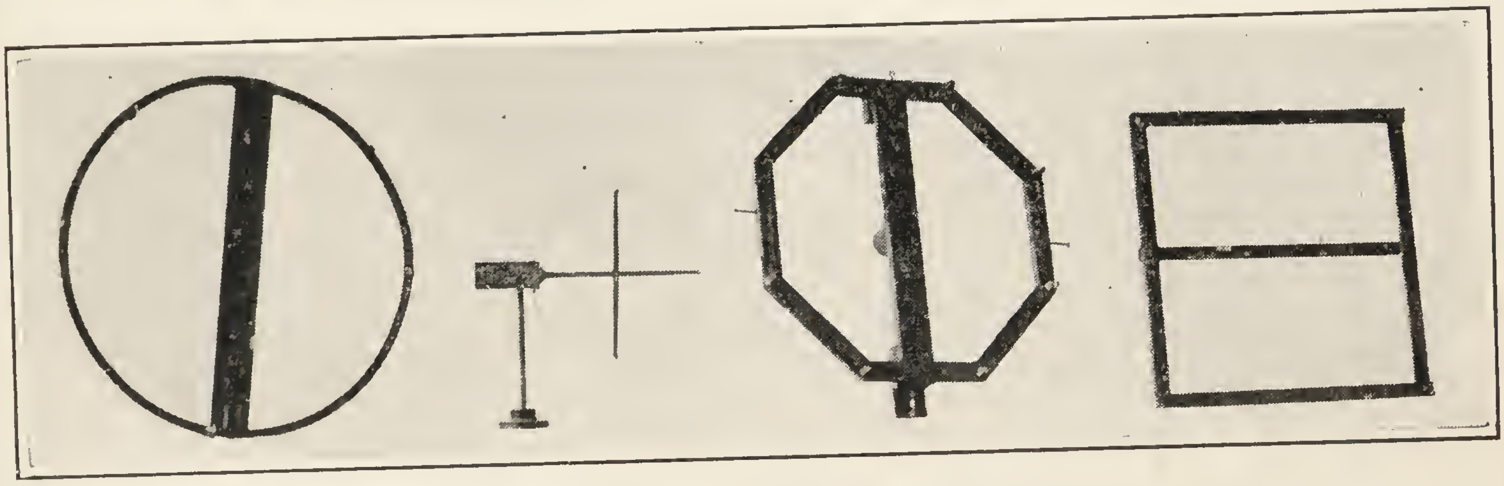


FIG. 4. FOUR RESONATORS USED BY HERTZ. Dimensions, left to right, 70, 35, 67, 60 centimetres.

similar construction, but he generally preferred a resonator of circular, square, or octagonal form, with a spark-gap, Fig. 4. The gap could be adjusted by a micrometer. Care was taken that the oscillator and the corresponding resonators should be in "tune" with one another. To get rid of spurious sparks

resulting from electrostatic induction, he occasionally interposed a wet thread between the electrodes of his spark-micrometer.

In most cases Hertz used as a detector the spark directly observed at the gap in a resonator, and measured by the spark-micrometer. Occasionally he employed a hot-wire galvanometer, and in some circumstances a suspended tube of gilt paper, Fig. 5. By such means, he surveyed space and determined the positions of nodes and anti-nodes, and the length and frequency of waves.

His investigations then extended to the study of reflection,

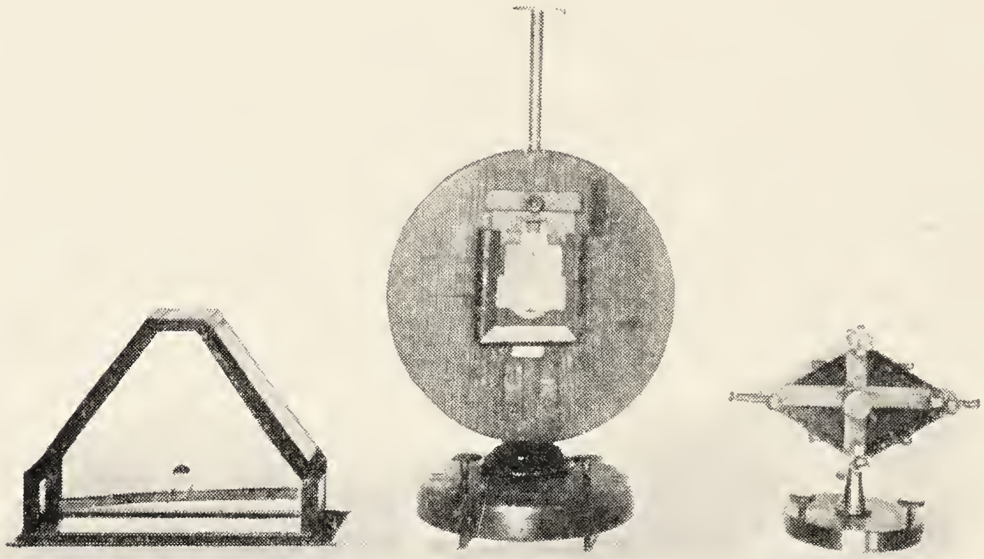


Fig. 5. Detector (at left), consisting of a tube of gilt paper and case 16 centimetres in height. Tangent Galvanometer. Height, 40 centimetres; diameter of disc, 22 centimetres. Hot-wire Galvanometer in lozenge-shaped case 17 centimetres high, 18 centimetres wide.

refraction, and polarization, in the optical sense, of electromagnetic waves. For this purpose the oscillator and the resonator were placed respectively at the principal foci of two parabolic mirrors of sheet zinc, each 2 metres in height, illustrated in Figs. 6 and 7. A wave length of about 50 centimetres was used. The receiver consisted of two thin brass wires, each about 52 centimetres in length, connected to a spark-gap outside the parabolic mirror. He obtained results up to a distance of about 16 metres; and he found that the waves could penetrate a wooden door, but not a zinc screen. The ordinary laws of reflection were proved by him to hold for electric waves. To examine the phenomenon of refraction he employed a prism of

pitch weighing about 800 kilogrammes. He stated that in passing from air into a solid transparent medium the action exhibits refraction like that of light, but that it is more strongly refracted than is visible light.

In the earlier experiments, his induction coil was 52 centimetres in length and 20 centimetres in diameter. It was operated by six large Bunsen cells, through a mercury interrupter, Fig. 8. Later he preferred a smaller induction coil, having a maximum spark length of 4.5 centimetres. It was operated by three accumulators. Sparks from 1 to 2 centimetres between the knobs of the primary conductor sufficed for many of his experi-

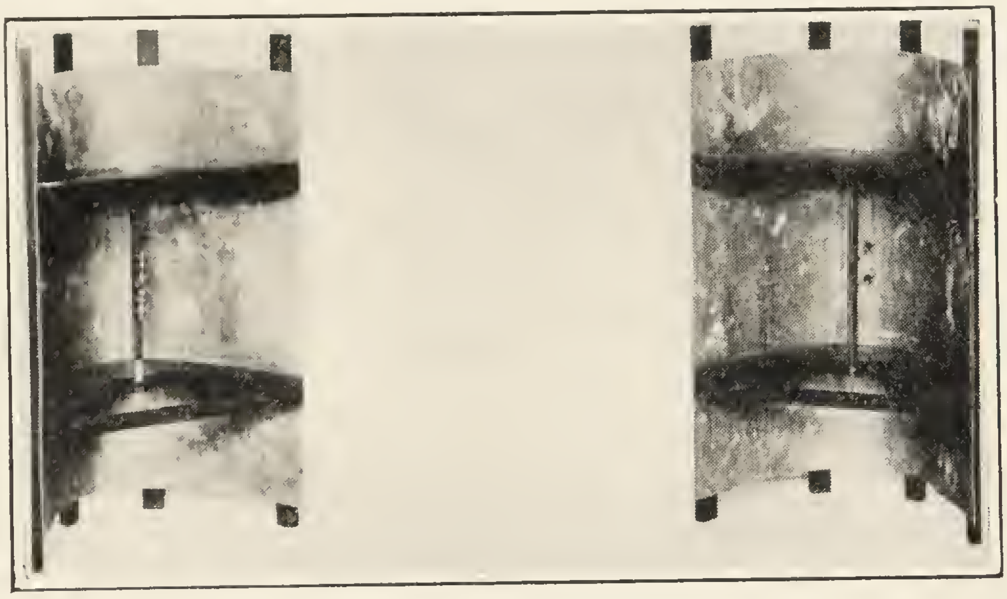


Fig. 6. Parabolic Mirrors 2 metres in height. One contained an oscillator and the other a resonator. (See also Fig. 7.)

ments. The spark-gap in the resonators was small—in some cases about 3 millimetres. With such apparatus he satisfied himself that the propagation is rectilinear. To demonstrate the phenomenon of polarization, in the optical sense, he utilized his mirrors in a manner similar to that applied to the polarizer and analyzer of an optical polarization apparatus. He made (Fig. 7) an octagonal frame, 2 metres high, across which he stretched copper wires 1 millimetre in diameter, parallel to one another, 3 centimetres apart. It behaved as a tourmaline crystal behaves towards a plane polarized beam of light.

Hertz soon confirmed the views of Heaviside and Poynting that the process whereby currents are induced in secondary circuits by such oscillations as he was employing takes place for

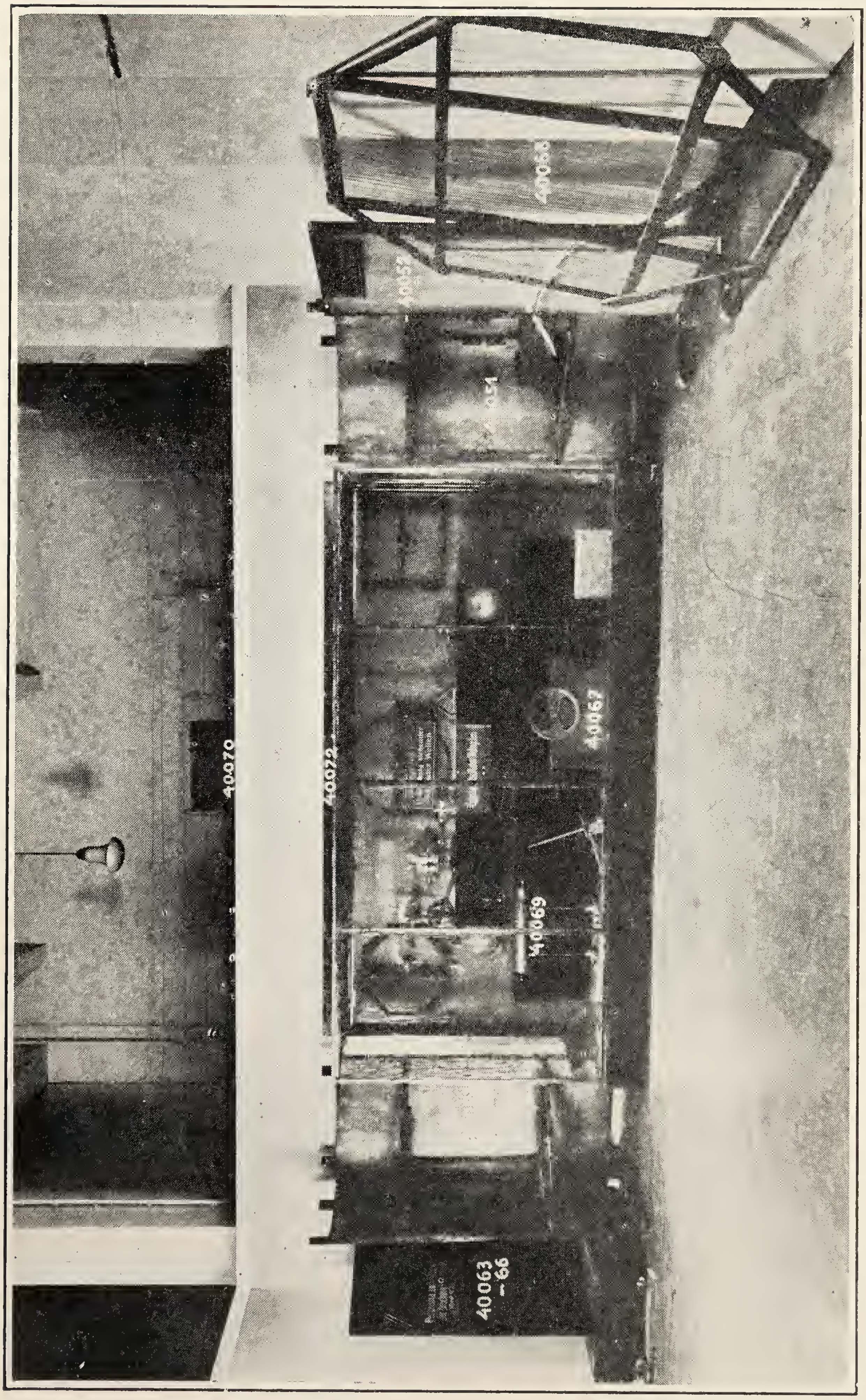


Fig. 7. Collection of Hertz Apparatus at the Deutsches Museum, Munich. No. 40050, parabolic mirror with resonator. No. 40051, parabolic mirror with oscillator. No. 40052, plane mirror, 2 metres high and 1 metre wide, of sheet zinc. No. 40063-66, pitch prism, height 1.59 metres, width 1.20 metres, weight 1200 kilogrammes, for refracting electromagnetic waves. No. 40068, octagonal grating of wires on frame 2 metres high, made by Hertz for experiments on "polarization". No. 40069, cylindrical conductor upon two glass supports, used for varying the capacity of circuits. No. 40070, "squirrel-cage" consisting of 24 copper wires, each 5 metres long, forming a cage about 30 centimetres in diameter for experiments on stationary electric waves. No. 40072, wooden trough, 21.5 centimetres wide and 45 centimetres in length, for experimenting with electric waves in liquids.

the most part in the surrounding dielectric, and that the interior of secondary conductors plays scarcely any part. As the action from the primary in his experiments traversed the air point-to-point to the secondary, propagation was there of necessity concerned with the dielectric. At high frequencies a metallic sheet, placed round the secondary, effectively screened that circuit. He then asked, to what extent may the thickness of the sheet be reduced? Such tinfoil as was available was thick enough to prevent penetration; so was gilt paper. Below the mere surface of the conductors, therefore, all was complete calm. He

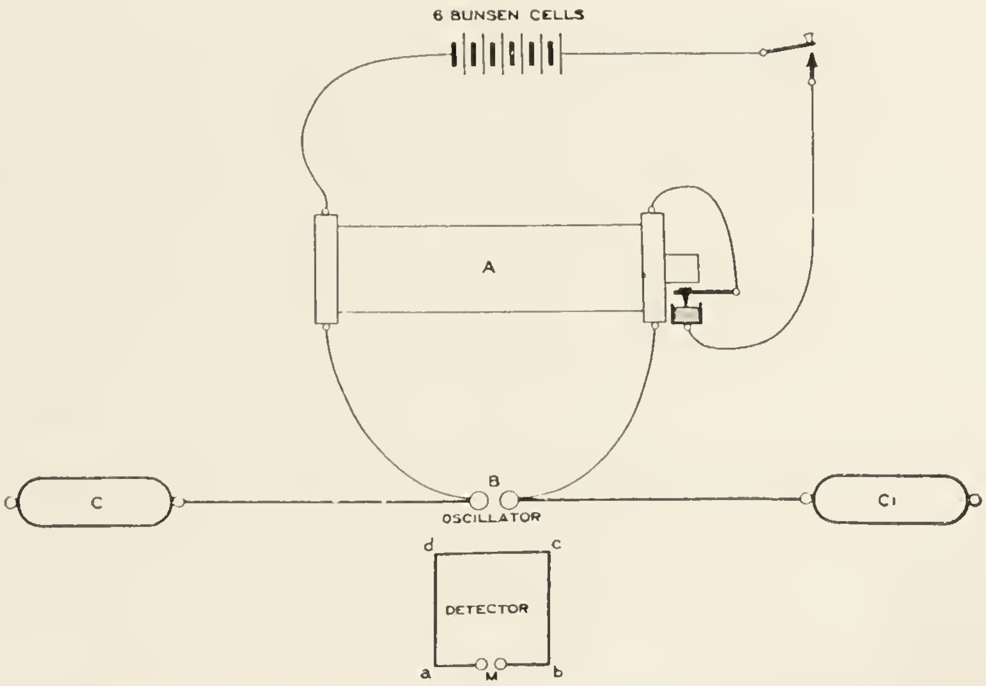


Fig. 8. General Arrangement of Hertz Oscillator and Resonator.

deduced that such electric waves scarcely penetrate further than does the light reflected from the surface of a conductor.

Near one of the end plates of his primary conductor he then placed a conducting plate and fastened to it a long straight wire. In his earlier experiments, he had already shown how the action of the primary oscillation could be conveyed to great distances by such a wire. He now demonstrated by suitable screening that all the effects are restricted to the surface and to the space outside such a wire, and that the interior knows nothing of the waves. He went so far as to try screening with glass-tubes chemically silvered, and at last, with this screen immeasurably thin, it was possible to penetrate the conducting barrier. The sparks at the detecting gap within appeared only

when the film of silver was no longer opaque to light, and when it was certainly thinner than $\frac{1}{1000}$ millimetre. Like results were obtained by screening the primary. Care had to be taken, however, to avoid openings at the ends and elsewhere in the screens. It was in the course of these experiments that he constructed the apparatus for stationary electric waves, resembling an elongated squirrel cage (Fig. 7, No. 40070) 5 metres long and 30 centimetres in diameter. It was built up of twenty-four copper wires extended parallel to one another along the generating cylindrical surface, over seven rings of wire equally spaced. It was provided with a central wire, and he constructed a very small exploring resonator, with an adjustable spark-gap, for examining the distribution of nodes and anti-nodes within the cage. Thus he satisfied himself, and thus he propounded the paradox that

> Whereas all propagation of electrical disturbances takes place through non-conductors, conductors oppose this propagation with a resistance that in the case of rapid alternations is insuperable.

Then reverting to the idea of electrical "polarization", he regarded the oscillations as being so rapid—he was dealing with about a hundred million a second—that the quantities of electricity displaced in insulators by such "polarization" are of the same order of magnitude as those set in motion by conduction in metals. When iron wire replaced copper, he obtained similar results and thereby confirmed his supposition that with iron the magnetism cannot follow such exceedingly rapid oscillations. He expressed regret that he had no experimental data with regard to how the discharge of Leyden jars is affected by the presence of iron. Probably he conceived that such discharges, owing to the comparatively great capacity of the jars, would be of considerably lower frequency than discharges from his ordinary oscillator of very small capacity.

To increase the self-induction of the resonating circuits he introduced loose spirals, Fig. 9. He also examined the effect of using wires of different metals, particularly of iron. Conductors such as those illustrated in Fig. 7, No. 40069, were for modifying the capacity of his circuits. Occasionally he explored the

circuits for nodes by spark-gap tests carried out by the aid of a small insulated metal sphere brought near to the conductors at

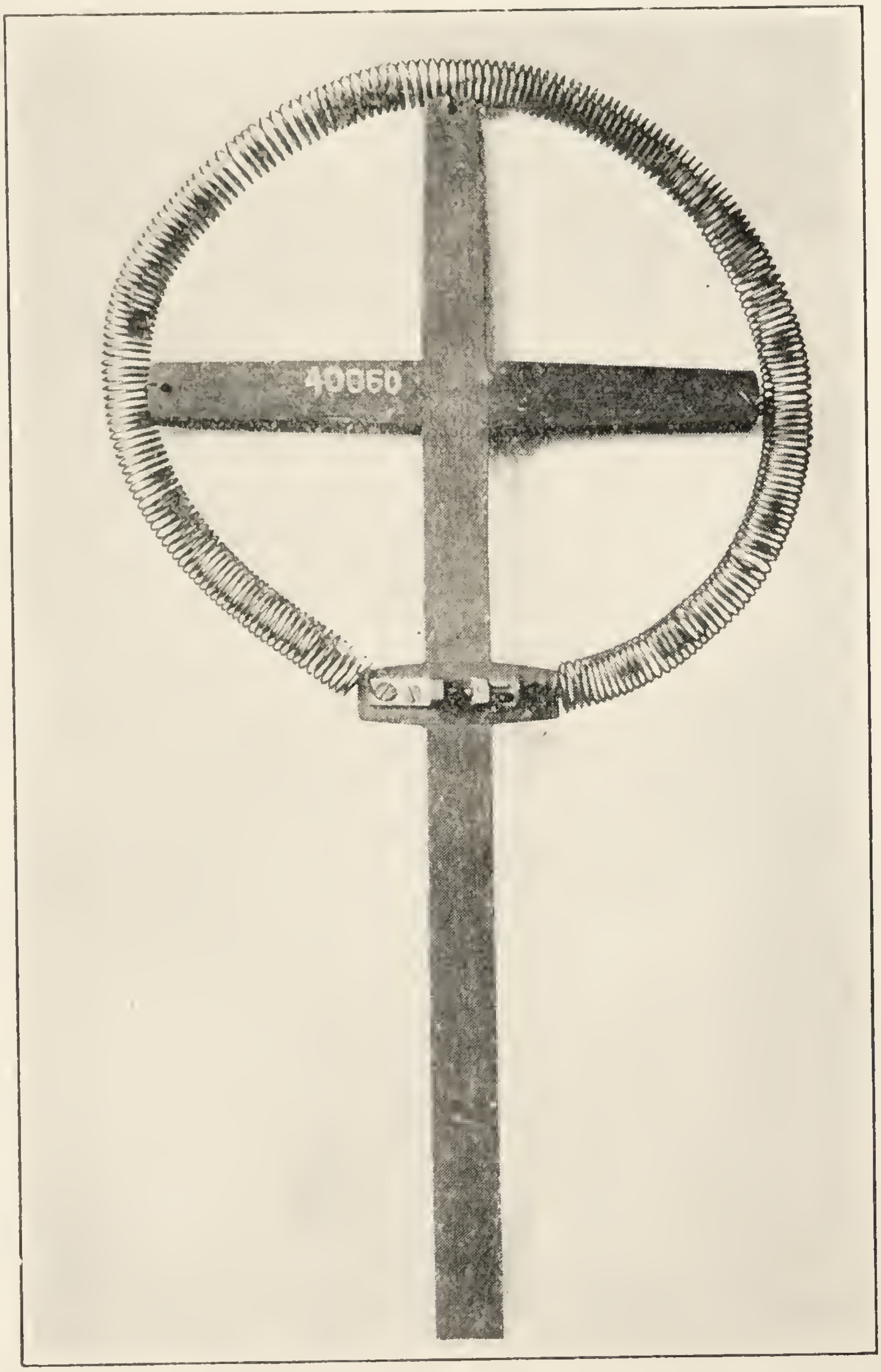


FIG. 9. CIRCULAR FORM OF RESONATOR, 21 centimetres diameter, of brass wire wound into a spiral to increase self-induction for long waves. The micrometer spark-gap is here illustrated.

various points. It was by this means that he first became aware that "overtones" were present, and that irregular effects were superposed upon regular effects.

From the tangled threads of growing knowledge concerning these irregular effects he proceeded to weave a new research. He observed that the sparks at the gaps of the exploring resonators were influenced by light from any neighbouring sparks—for example, by light from the originating spark at the induction coil. He was in this manner led to discover the influence of ultra-violet light upon electric discharges. The original apparatus employed by Hertz at Karlsruhe to demonstrate this effect is illustrated in Fig. 10. A vacuum is first formed in the

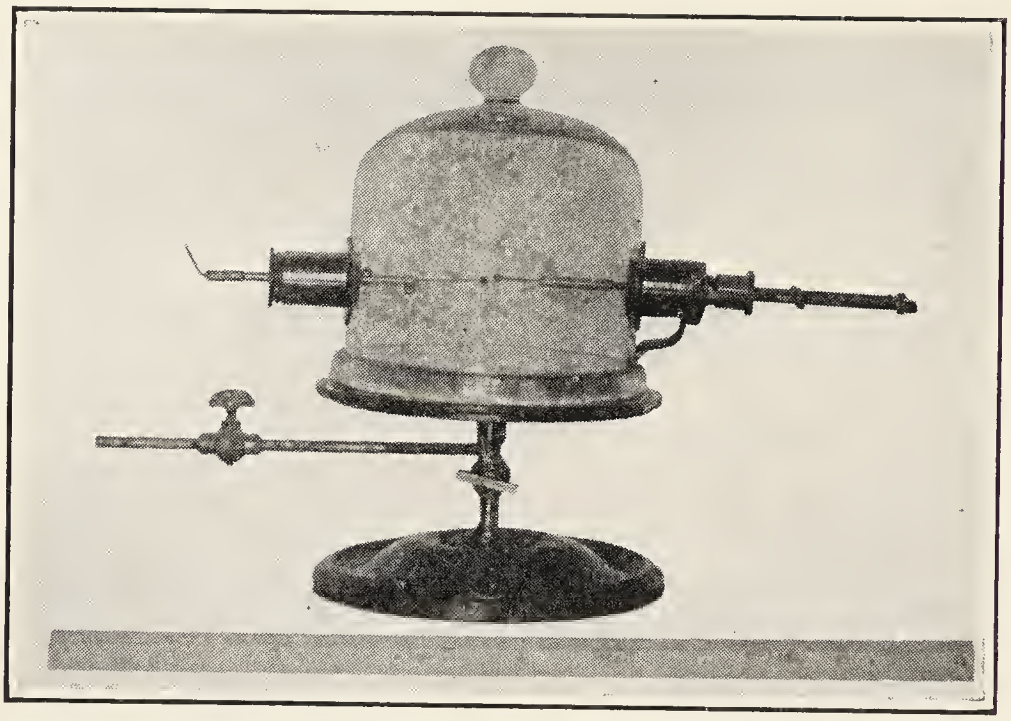


Fig. 10. Original Apparatus used by Hertz at Karlsruhe to illustrate the effect of ultra-violet light on electric discharge.

receiver by an air-pump, and the spark-gap is adjusted so as to be somewhat too long to allow the discharge, with ordinary illumination, to take place. Ultra-violet light, from another spark or from some other source, is then allowed to fall upon the gap, ionization consequently occurs, and the spark passes. Screening of the spark-gap from ultra-violet light accordingly diminishes the maximum spark-length in a resonator corresponding to any given arrangement of the resonator. These results were published by him in June, 1887.

In the summer of 1887, Hertz studied the influence that he thought might be exerted by dielectrics upon electromagnetic waves. His usual mode of investigation was to examine the

effect of the presence or absence of a block of insulating material upon the position of the neutral point in one of his radiators. The form of his original apparatus for this purpose is illustrated in Fig. 11. He subsequently regarded this work as fruitless—his efforts being frustrated by the occurrence of subsidiary sparks. Ultimately he followed a new line, and was rewarded by finding that the distance to which he could detect electro-

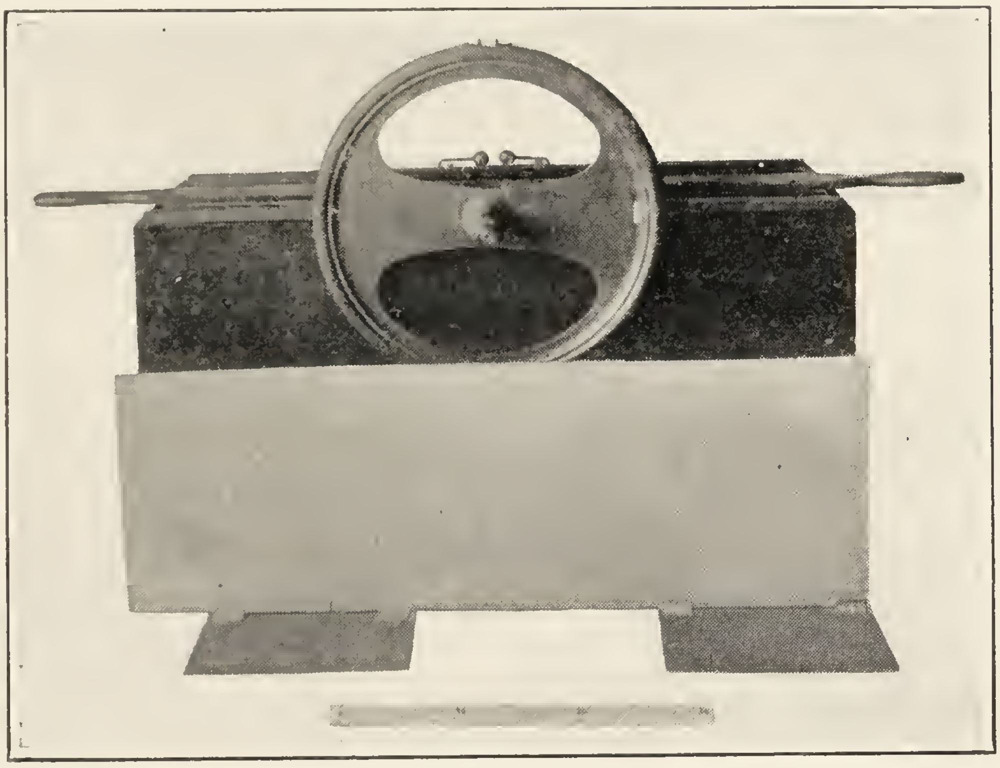


Fig. 11. Wooden Box containing a Block of Paraffin Wax, $70 \times 32 \times 18$ centimetres. Resting on the box are rectangular brass plates forming an oscillator, the spark-gap of which is seen through the aperture. The oscillator was raised or lowered on to the block to observe the effect, if any, of the presence of the dielectric (wax). The circular apparatus carries a wire, forming a resonator, the small spark-gap of which is seen at the top. In front is a metal sheet, used by Hertz as a plate of a condenser.

magnetic waves might be extended. Distance in fact was not determined by the Newtonian law of inverse squares. He pressed on, and succeeded in transmitting up to the then enormous distance of 12 metres. His experiments on the reflection of waves were completed in March, 1888. The summer of that year was given to the study of the propagation of waves by means of wires, and in September, 1889, he delivered at Heidelberg his famous lecture on the relations between light and electricity.

To obtain an idea of the conditions under which the earliest

experiments in Hertzian wave transmission were carried out, a visit was paid to Karlsruhe (Baden). By the kindness of Professor Gaede, who now (1927) occupies the professorship held by Hertz, the Hertzian apparatus was reassembled in the lecture theatre of the Physikalisches Institut of the Technische Hochschule, in the positions where Hertz originally arranged them, and the photograph reproduced in Fig. 12 was obtained to illustrate the present account of the experiments. From left to right, there is first seen upon a small table some bichromate cells, and the original induction coil used by Hertz. Then appears the parabolic mirror of sheet zinc containing the *Sender*, or oscillator. Next to this is the pitch prism, and then the second mirror containing the *Empfänger*, or receiver. These mirrors are exact duplicates of the originals, and were made by the Institutsmechaniker, Amann, who constructed the originals for Hertz. The originals are at Munich (Figs. 6 and 7). To the right of the *Empfänger* in Fig. 12 is the wire cage for experiments on skin-effect, and on the extreme right is the plane mirror of sheet zinc. In the foreground on the right is a corner of the lecture table. The preparation room is approached by a door at the back of the lecture table. An inscription on the wall of this lecture theatre, not visible in Fig. 12, reads:

> In diesem Saale stellte Heinrich Hertz seine ersten Versuche mit Elektrischen Wellen an.

It was also in 1889 that Hertz was appointed to succeed Clausius and Ketteler as ordinary professor of physics at the University of Bonn. At Bonn he had as colleagues Breisig, Lenard, and his own brother-in-law, Carl Pulfrich. He there set before himself the task of building up a simple electromagnetic theory upon the principles of Maxwell, as confirmed by his own experiments. He also directed his attention to the discharge of electricity in rarefied gases; an appropriate research, for Bonn had already to its credit the achievements of Hittorf, Geissler, Kayser, Pluecker and others, and a laboratory scintillating with original apparatus—the weapons of their victories in this field. Pluecker in 1859 had studied the fluorescent effect that cathode rays produce upon the glass walls of a discharge tube; Hittorf,

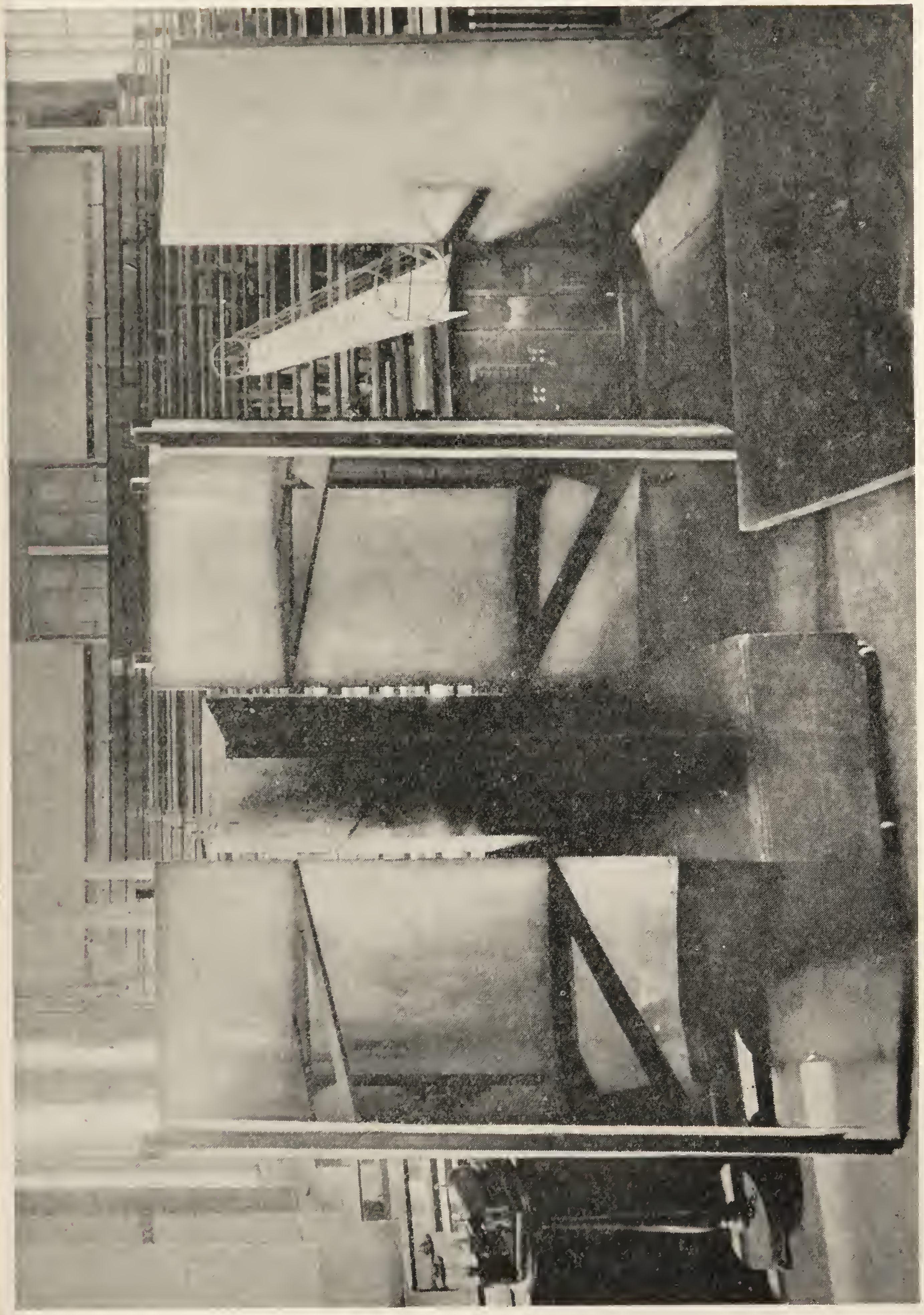


Fig. 12. The Lecture Room at the Physikalisches Institut at Karlsruhe, with apparatus as arranged in the experiments by Hertz in the years 1885 to 1889, that led to his discovery of Hertzian waves.

Goldstein, Puluj, and Crookes had continued the investigation. Hertz gave new direction to scientific thought. He experimented with thin plates of metal fixed inside a discharge tube, and he was rewarded by the discovery that cathode rays can be made to pass through metals. Then his colleague, Professor Philipp Lenard, conceived the idea of inserting such a metal plate in the glass wall of the tube, as a "window" through which the cathode rays, imprisoned within the glass, might escape. They did. The rays stole out into the open and assumed disguise as "Lenard rays". Lenard is said to have given one of these tubes to Röntgen of Würzburg. In the autumn of 1895, Röntgen was experimenting with invisible light—from a discharge tube enclosed in black paper. He discerned the penetrative power of rays that proceeded from the bombarded regions, *i.e.* from the regions of impact of the cathode rays against the interior surfaces of the discharge tube in which they were generated. So nearly did Hertz and Lenard find the little more that was to be so much. The sequence of discoveries by Pluecker, Goldstein, Hertz, Lenard, and Röntgen must remain an example of the ease with which observers can hit or miss what may be just behind the surface of achievement in natural science.

Most of the discoveries with which the name of Hertz is associated were made known originally in *Annalen der Physik und Chemie.* These writings were collected by him. After adding some explanatory notes, and a dedication to von Helmholtz, he published them in 1891 as a treatise, with the title *Untersuchungen über die Ausbreitung der elektrischen Kraft.* This book was admirably translated into English by Professor D. E. Jones, and a preface to the English edition was written by Lord Kelvin. The English title of it was *Electric Waves, being Researches on the Propagation of Electric Action with Finite Velocity through Space.* Later there was issued *Miscellaneous Papers,* with an introduction by Professor Philipp Lenard. These papers included an account of the earlier investigations, and brought together writings otherwise difficult of access. The work was translated into English by Professor D. E. Jones and Professor G. A. Schott. To complete the collection there was also issued

Hertz's *Principles of Mechanics*. The whole series was edited by Professor Lenard. Additional notes of a biographical character are to be found in the address given in 1894 by Professor Franz Richarz to members of the University of Bonn, and in some brief writings of Dr. F. Breisig. Useful particulars concerning the relics of apparatus are contained in a short article by Dr. Franz Fuchs in *Der Radio-Amateur* for April, 1926.

The closer the early papers are studied, the greater becomes the amazement that Hertz who had applied himself seriously to mathematical physics for rather less than two and a half years should have obtained so quickly such a grasp of the most intricate part of the subject. He himself confessed to the inestimable value of the inspiration that von Helmholtz imparted to him and to other pupils, to the pre-eminent fitness that von Helmholtz possessed for guiding them in original research, and to the value of this personal influence, that led them to see things as part of all creation instead of as separate entities. This guidance by von Helmholtz was free to all who came under the care of that illustrious and beneficent philosopher—it could direct but could not evolve such genius as that of Hertz. The truth is that Hertz possessed remarkable powers of sweeping away from elaborate theories everything but the part needed to assist in attaining his object. This hint he has left us. The remainder of the secret was bequeathed by him as a theme for research students for all time.

In his philosophy, Hertz was in all respects a Newtonian. For him, the group constituting electricity, magnetism, and light belonged to mechanics. The difficulty was, as he confessed, that the mechanics of that group had not been completely disclosed to human understanding. In his earliest work (1886–87) he had not perceived that the laws of electricity applicable to the steady state differ from those of electricity in motion. When he realized the distinction, he immediately advanced. He first ascertained that the oscillations with which he had to deal were characterized by variety and regularity. Then, out of confusion came order; but it was not the old order, for the amplitudes of these regular oscillations decreased more slowly than inversely as the square of the distance from the

source. Next, the distances traversed by the electric waves surprised him, even with his crude means of detecting them. Moreover, the waves had finite velocity. He then knew that the immemorial doctrine of "action-at-a-distance" must die, and that the Faraday-Maxwell theory, which had not yet been fully appreciated in Germany, must supersede it.

Hertz saw that Maxwell's theory involved three assumptions, all relating to the dielectric medium through which the electric and magnetic forces are exerted. In effect, he said, consider the space between the two plates of a simple condenser. Apply a battery suddenly to the terminals. There is an initial current through the dielectric—Maxwell's displacement current—but not a continuous steady current. The electric stress has produced in the condenser an electric strain, a strain not exceeding the limits of the tenacity of the dielectric. Maxwell had assumed:

(1) That such changes of electric stress correspond to the same electromagnetic forces as do the steady currents equivalent to them.
(2) That forces, electromagnetic as well as electrostatic, are able to produce dielectric stresses.
(3) That air and empty space behave like all other dielectrics.

Hertz then deduced, from Maxwell's equations, that if (1) and (2) are correct, waves of the kind expected by Maxwell could be propagated in any given dielectric with finite velocity, not necessarily the velocity of light. Hence, if a wave could be shown to be propagated at finite velocity in air, (1) and (2) might be proved to hold. In this demonstration, after some preliminary failures, he succeeded. Then he investigated stationary waves along straight wires, their nodes and anti-nodes, their wave lengths, their phases and their phase-differences, their reflections and their mutual interferences. At about this time he became for once disheartened—the rate of propagation seemed to be infinite, and the phase of the interference seemed different at different distances. Was Maxwell's theory then false? He left the matter alone for a few weeks, and then tried again. His genius had left him desolate. He was moreover disturbed

because the account of his research was marred by an error of calculation—to which Poincaré had drawn attention. Further, could it be that electricity possesses inertia, after all?

Discouragement did not restrain him from research. Freeing himself from preconceived notions, he repeated his experiments and found that the set of wave interferences which he had observed could not be harmonized upon the assumption of infinite velocity. He now thought that velocity in air must be finite, and greater than that in a wire. Then followed misgivings concerning the possible effect of the neighbouring walls, the iron stove, and other obstructions. It is said that at this time he remarked upon the benefits that might result if science had available an enclosed space comparable with that of Cologne Cathedral. During this period he discovered that for long waves the velocity was greater in air than in wires, but for short waves he found practical equality. He declared the result to be so surprising that he could not accept it as certain. His doubt arose once more from the environment, for, he reasoned, if long waves cannot be developed they cannot be observed. He knew that the velocity of the wave in any case depended upon the reciprocal of the square-root of the product of specific inductive capacity and permeability. He also knew that, at such high frequencies as he was using, only the surface layers of conductors come into play. These facts, however, did not completely satisfy him. Again he appealed to experiment. In the autumn of 1888, while investigating waves in the narrow interspace between wires, by means of small detectors, he found distinct nodes at or near the ends of the wires, even when the detectors were very small. This brought him on to the track of very short electric waves and enabled him to repeat, with waves only 30 centimetres in length, all his earlier experiments. He found that, in wires, such waves travel with very nearly the same velocity as in air. But he was still dissatisfied, for his view was that an experiment carried out under proper conditions would prove not only that the velocity in air is finite, but that the velocity in air and in wires is equal. Also he was influenced by the teaching of von Helmholtz that a distinction must be maintained between the electromagnetic and the electrostatic forms

of force, and that until the contrary is proved they must be assigned two different velocities. He sought to establish the true relationship between the two forms by observing the mechanical forces exerted by the waves upon ring-shaped conductors.

It is well to remember that these famous experiments were carried out with apparatus simple and inexpensive. In the physics department at Karlsruhe and at Bonn were indeed precious relics of the preceding age, but Hertz converted to his purpose the ordinary contrivances in metal, wood, glass, silk, tin-foil and sealing wax that might have been found in any university laboratory of his time. What further was needed he made, after the manner of the pioneers, with his own hands, out of bits of wood and wire and sheet-zinc, so that all was infinitely adjustable.

The error that crept into the calculations relating to his work, and that caused him such annoyance, was of a subtle kind. He was determining the time of oscillation of his usual form of oscillating system, consisting of a straight conductor with a sphere at each end, as in Fig. 3. The expression for this time involves the square-root of the capacity of one of the equal spheres. When the spheres are far apart, the charge on each is the product of this capacity and the potential difference between the sphere and the earth. In his calculation he took the potential difference to be that between the two spheres. This was equivalent to calculating the capacity of one of the spheres to be 15 instead of 15/2. The effect was that he estimated the required time of oscillation as 1.77 instead of 1.26 hundred millionths of a second. That so great a philosopher should have been thrown into despondency concerning a matter of only 5.1 thousand millionths of a second is probably unique in the history of human effort. Even after Poincaré had placed the fine point of his cambric needle upon this error, Hertz was not content, for in 1891 his own comment was:

> Abgesehen davon, dass in der Rechnung der eben erwähnte Fehler zu corrigiren wäre, ist auf die Dämpfung durch Strahlung keine Rücksicht genommen, an welche ich bei Abfassung dieser Abhandlung noch nicht dachte.

In other words, he accused himself, in addition, of having neglected the chance losses by radiation. These radiation losses have since received the attention of profound mathematicians including Larmor, Lorentz, Planck and Schott, and the outstanding fact remains that, as Hertz realized, a rigorous method of evaluating losses by radiation has yet to be found.

To derive from the life and works of Hertz the greatest help and encouragement, the investigator must realize how completely he combined those qualities of a philosopher that make an observer, a logician, and a mathematician. Although he derived his primary inspiration from the mathematics of von Helmholtz and Clerk Maxwell, he was able at will to free himself almost completely from the allurements and restrictions of symbolism. By direct experiment he proved that the propagation of electric waves through space is an affair of time as well as an affair of distance—a point-to-point process through a continuous entity that can undulate. He thus disposed of the theory of electrical action-at-a-distance, *i.e.* the theory that electrical actions, like thought itself, can spring instantly across space. Following Faraday and Maxwell, he taught that electrical and magnetic forces can disentangle themselves from inert bodies, and that they can continue to exist as wave conditions or changes in the state of the medium. Finally, he confirmed by experiment the close correspondence between electric and magnetic forces and light.

Between the teachings of the English and German schools of thought he was in a position of some delicacy. He saw that with the acceptance of Maxwell's idea, the physical basis of von Helmholtz's theory must disappear—for action-at-a-distance would become intolerable. He had therefore to go carefully through Maxwell's work step by step, stumbling as others had done "upon unwonted mathematical difficulties", and again as others had done abandoning hope of forming for himself an altogether consistent conception of Maxwell's ideas. "What, he asked at last, is Maxwell's theory? Maxwell's theory is Maxwell's system of equations." The recognition of this fact enabled him to be up and doing, and to follow further the injunction of Maxwell, to sweep cobwebs off the sky.

His analysis began with scrutiny of the four theories that in his day held the field, relating to the attraction between two electrified bodies. Briefly, the first of these presupposed a kind of spiritual affinity. It recognized that the force exerted by the first body is intimately associated with the presence of the second body, thus resembling the attraction between two magnets. Here was primitive action-at-a-distance, naked and unashamed. It was consistent with Coulomb's law. It held for gravitation. With what happened in empty space it had no concern. For electricity, this theory had to be abandoned. The second was also of a pseudo-spiritual character. It too applied to the case of two bodies, but each body strove to attract in all directions with forces of definite magnitudes, even if no other bodies were present to respond. Space was filled with strivings that varied from consecutive point to consecutive point of a vague universe. Each body was at once the seat and the source of forces. Whether space was full or empty was immaterial. Hertz considered that Maxwell adopted some but not all of this second theory. The third was a development of the last. The attraction between the two separate bodies was not in this third theory determined solely by forces operating in accordance with the principle of action-at-a-distance. Space came into it. Space was no longer empty. The forces induced changes in the condition of space. These changes gave rise to new action-at-a-distance forces. Electricity and magnetism implied here the "polarization" of each of the smallest parts of space matter, and this "polarization" was brought about by the forces. The attraction between the two electrified bodies depended partly upon the influence of changes in the medium. Similarly, the energy was, in general, partly in the electrified bodies and partly in the medium. In the limiting case of this theory, the whole energy was in the medium, and there was no so-called "free" electricity; consequently in the limiting case the action-at-a-distance force vanished. Hertz saw that this limiting case led to Maxwell's theory, although the physical ideas were, in his view, not Maxwell's. The fourth theory transferred the action entirely to the agency of the medium. The changes in the medium—contemplated in the third theory—were present in

the medium in the fourth theory, but in the fourth there were no action-at-a-distance forces to cause "polarizations". Yet "polarizations" were the only means of action present. They caused the movements of the attracted bodies. How the "polarizations" were formed was unknown—consideration of the physics of that matter was to be deferred. Thus, according to Hertz, did Maxwell view the physical universe, thus did Maxwell discard action-at-a-distance, and upon the foundation of this fourth theory did Maxwell build his System of Equations.

As a mathematician, Hertz, possessed more than ordinary knowledge and skill; his mind was as clear as crystal, his methods of attack were direct, original, penetrating. He operated with conspicuous courage. As an example may be taken his method of treatment of a spherical wave in a medium that is homogeneous except near the origin of co-ordinates. Lord Kelvin singled out for special praise his method of dealing with the problem of the elastic contact of two spheres. Hertz at first found difficulty in accepting the corpuscular theory of cathode rays, *i.e.* the theory advocated by Stokes and Crookes, and now confirmed, that they consist of particles projected from the cathode, in straight lines, at high velocities. He could not readily imagine corpuscles penetrating metals, and accordingly he was, for a time at least, inclined to interpret cathode rays as waves.

The laboratory in which Hertz worked at Bonn was in part of the ancient palace of the Prince Elector. At the time of the Roman-German empire the neighbouring city of Cologne was one of the Electoral principalities, and the Prince Elector was also Archbishop of Cologne. But Cologne, being a free city, did not permit the Archbishop to reside there except for clerical functions. Because no palace was found for him in Cologne, in accordance with the predilections of princes of Bavaria for castles in those days, he provided himself with one at Bonn. From the year 1320 this was the constant residence of the Archbishop of Cologne. In the nineteenth century, when the Rhineland went to Prussia, the University of Bonn was endowed with the archiepiscopal palace. Clausius resided there,

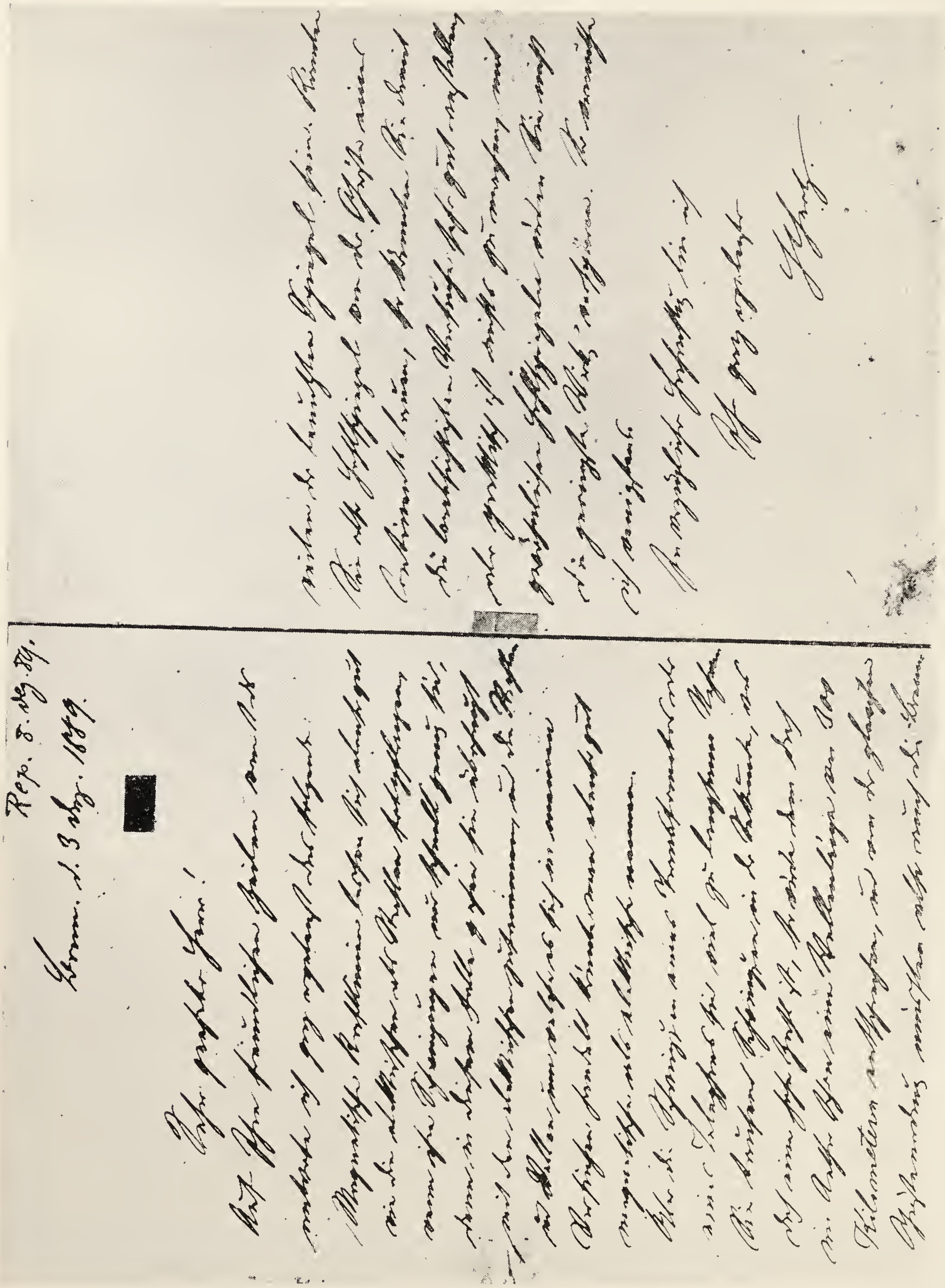


FIG. 13. HERTZ'S LETTER TO HERR HUBER, reproduced by courtesy of the Deutsche Museum, Munich.

TRANSLATION

Bonn, *Dec. 3rd*, 1889.

Dear Sir:

Replying to your kind letter of 1st, I have pleasure in giving you the following particulars:

Magnetic lines of force may be propagated just as well as electric, as rays, if their vibrations are sufficiently rapid; in this case they proceed together, and the rays and waves dealt with in my experiments could be designated magnetic as well as electric.

However, the vibrations of a "Transformator" or telegraph are far too slow; take, for example, a thousand in a second, which is a high figure, then the wave length in the ether would be 300 kilometres, and the focal length of the mirror must be of the same magnitude. If you could construct a mirror as large as a Continent, you might succeed with such experiments but it is impracticable to do anything with ordinary mirrors, as there would not be the least effect observable.

With kind regards,

Yours ———

Hertz.

but Hertz preferred to have a private residence elsewhere in the vicinity.

At Bonn, in July, 1892, Hertz was attacked by a pernicious illness, attributed to a carious tooth that caused ulceration of the upper jaw. Repeated operations, a sojourn in the Riviera in the spring of one year, and a holiday at Reichenhall in the autumn of the next, so improved his condition that the danger was believed to have passed. General symptoms of blood-poisoning next showed themselves, and extended to the bones. Nevertheless, until the middle of December, 1893, he delivered his lectures and he prepared new experiments. The disease, however, reasserted itself, and on January 1, 1894, he died.

Amongst those who knew him best, the remembrance that remains of him is of a mar of amiable disposition, social, genial, a good lecturer, possessed of singular modesty, who gave himself no airs as of a great professor, and who, even when speaking of his own discoveries, never mentioned himself. When the Royal Society presented him with the Rumford medal, he silently disappeared from Bonn for a few days—none knew why—and he returned as silently. The habit he formed early in life of solving difficulties for himself continued with him; he preferred, upon occasion, to puzzle things out in loneliness in the laboratory. His decision to follow pure science instead of a technical career was faithfully kept, and yet the importance of the part he played in the ultimate technical advance in electrical science is beyond measure. It can be definitely stated that concerning the future employment of Hertzian waves for telegraphy and telephony he had no premonitions. For there exists a letter written by him to one, Herr Huber, who wanted to know whether there was a prospect in that direction. Hertz regarded it as impracticable. His reply, which is reproduced in Fig. 13, was to the effect that the application of such a mode of electrical communication to practical telegraphy or telephony would need a mirror as large as a continent.

Hans Christian Oersted at the Age of 26—From an Engraving by Chrétien, Paris, 1803.

ODE BY HANS ANDERSEN.

Til H. C. Oersted.

Da Tanke-Lynet udsprang fra Din Pande,
En stoerre Seekraft Videnskaben fik,
En umaalt Skat Du gav til Verdens Lande,
Og gjennem alt det Skjoenne i det Sande,
Til Gud Du foerer os med aabent Blik.

To H. C. Oersted.

When to thy mind there flashed the lightning thought,
The realm of Science, wondrous in the blaze,
Revealed such treasures in the Truth you taught,
That men before its Beauty bowed, and sought
A path to its Creator, by your ways.

R. A.

VI

HANS CHRISTIAN OERSTED

In the history of electrical science, the first twenty years of the nineteenth century constitute the period of transition from static to current phenomena. Following upon the work of Galvani and Volta, discoveries throughout those two decades manifested themselves in all the advanced countries of Europe. In Denmark, however, at the end of that time, what had been the dream-phantasy of electrical communication suddenly took shape, inspired hope, gave confidence, and urged mankind to move towards reality; for in 1820, at Copenhagen, the secret now known as electromagnetism was wrested from Nature by Hans Christian Oersted.

He was born in 1777 at Rudkjöbing, a small town on the island of Langeland, where his father was an impecunious apothecary. In 1778 a second son, Anders, completed the family for the time being, and when, in due course, the problem of education for the two boys presented itself, there were difficulties—schools in Rudkjöbing were scarce, teaching was crude, and the Danish language did not suffice for prospective needs and aspirations. The apothecary proved himself a man of resource. He enlisted the services of his friend Christian Oldenborg, a Teutonic barber and wig-maker of slender means, to teach the lads elementary German. The aid of the barber's wife was sought also to help them to learn to read and to write. Arithmetic was at first an embarrassment, for the barber's knowledge in that direction was limited to addition and subtraction. Happily, a friendly schoolboy disposed of this trouble by imparting the rudiments of multiplication and division. From the baker they learnt a little drawing; from the burgo-

master, a smattering of French; and from the local surveyor, some mathematics. Moreover, when Hans was twelve years of age, he assisted his father in the apothecary business, and thus acquired the beginnings of chemistry. A few years later, he gained increased access to books and developed an inclination towards literature. Notwithstanding privations, the two boys studied hard; their intelligence was above the average, and their parents endeavoured by every available means to assist them to materialize their aspirations. Hans and Anders were so successful that in 1794 they passed the students' examination and six months afterwards entered Copenhagen University. There they derived a little support from State funds, and made up the remainder by teaching. At that time, as physics and chemistry were not accepted by the University for a diploma, Hans directed his attention to pharmacy. In 1797 he passed in that subject with honours. He gave attention also to physics and astronomy. As his brother's ambition was to be a jurist, it was possible for them to study together certain subjects appertaining to both of their respective professions. They shared one lodging and, for a time, they shared one amusement—their scientific work. They shared expenses. They also shared one friend—the poet Oehlenschläger. The poet had one sister. She married Anders.

At the age of twenty, H. C. Oersted showed definitely his bent towards literature and philosophy. So far did he improve his acquaintance with these subjects, that in 1797 he secured the University prize, a gold medal, for an essay "On the Limits of Poetry and Prose", and he won, in 1799, the prize for metaphysics.

His learning was becoming wide rather than deep. In his writings, early and late, may be discerned the influence of the German thinkers, Von Schelling (1775–1854, of Jena) and Kant (1724–1804, of Königsberg), whereby he was led to dwell upon the philosophy of Nature, to treat facts in poetical fashion, and to perceive, in the study of physics, something of romance and of estheticism. Thus was he taught to distinguish between the individual and the material system that surrounds the individual, to separate matters of knowledge from matters of

fact, sense from understanding, and mind from things, to seek to combine and to marshal in thought the scattered elements of experience, and generally to approach towards transcendental theology. On obtaining his degree as Doctor, this influence was recognized in his thesis, the title of which was "The Architectonicks of Natural Metaphysics", and it was destined to play a significant part in his subsequent career. To the present age, such philosophy appears fantastic, but whatever may have been its merits or defects, the attempt to master it gave activity and scope to his imagination and induced him to perceive or to dream of identity between all manifestations of energy, such as light, heat, chemical action, electricity, and magnetism.

The year 1800, memorable for the introduction of Volta's pile, discovered Oersted again doing service in an apothecary's shop, but not at Rudkjöbing. This establishment, which to-day still flourishes in Copenhagen, bears the name by which Oersted knew it—the Loeveapotheket (Lion Apothecaries). It was owned in 1800 by Professor Manthey, who happened to require the services of a pharmacist to whom he might entrust the business while he himself made a tour in Europe. Oersted, alive to the discoveries that had just been made by Volta, welcomed the opportunity this gave him for research. His first success related to the selection of solutions to produce the best results for the purposes of "galvanic electricity". He was impressed by the surprising results he was able to obtain by careful adjustment of the proportion of acid to water in the galvanic generator. He was also at work upon the classification of the earths and alkalis. In 1800, moreover, he took the Chair vacated by Manthey as Professor of Surgery. Reward for this labour came in the following year, when he gained at the University a money prize—the *stipendium cappelianum*—which supplied the means for him to travel for a few years at the expense of the State.

As soon as Manthey returned to Copenhagen, Oersted departed, in 1801, for Germany and France, filled with a desire to discuss pharmacy, metaphysics, and chemistry with men of light and leading. He found in Germany a realm of theory, in

Paris a paradise of experimental philosophy. The tour enlarged his sphere of activity, brought him into touch with his peers, taught him the measure of himself, and indicated the lines of progress. The friendships that were to prove of greatest immediate significance, however, were those of Winterl and of Johann Wilhelm Ritter (1776–1810) who at that time was living at Weimar.

The quality of Oersted's literary powers was at this time illustrated by an episode that reflected credit alike upon his ability and upon his generosity. In the years 1802–3 he was in Paris, and Ritter had just discovered the Ladungsäule (secondary cell), consisting of a stack of plates of a single metal separated by discs of cloth or cardboard damped with an inert liquid, and charged by a voltaic pile. With it, Ritter produced sparks, decomposed water and salt solutions, collected oxygen, hydrogen, and bases, and gave other remarkable evidences of its powers. Unfortunately, he could not speak, what he called, the "eigenthümlichdunklen Sprache" of the French, but he desired to compete for the annual prize of the French Institut in natural science. Accordingly, he prevailed upon Oersted to prepare a translation from German into French. This Oersted did so well that Ritter declared he could himself understand the translation better than his own original German.

On his homeward way from Paris, in 1803, Oersted visited Brussels, Leiden, Haarlem, and Amsterdam. At about that time, the University of Copenhagen was partly destroyed by fire, and the physical laboratory was burnt. The city fortunately contained a separate collection of physical apparatus, but it was held in private hands, and Oersted could not at first secure free access to it. By judicious borrowing, however, he assembled what apparatus he required for a series of public lectures on electricity, magnetism, heat, light, and combustion. His mind in these years was centred chiefly upon electro-chemistry, but he was still concerned with the identification of general laws, exemplified in all branches of natural philosophy. He was a keen observer, and gradually he developed into an enthusiastic experimenter. His experiments on tone-figures, carried out in 1806, proved to be of sufficient importance to lead to his

appointment as extraordinary Professor of Physics at the University of Copenhagen. This increased the facilities at his disposal for further investigations. He became a teacher also at the Military School, and gave lectures to the general staff. In 1809 he produced a textbook of mechanical physics, which was republished in an enlarged edition in 1844.

In 1812–13, Oersted made another tour in France and Germany. He remained a long time in Berlin, and published there, in German, his researches upon the identity of electric and chemical forces, which had already appeared in French in Paris. Upon his return to Copenhagen in 1814, he married, on May 17, Inger Birgitte Ballum, who was born on March 28, 1789, and who died on November 3, 1875. She was a daughter of N. R. Ballum, a pastor of Kjelby on the small island of Moer. At about the time of his marriage, he associated himself with a movement to introduce into chemical terminology the Gothic and German languages to displace the Greek and the French. In addition, he sought to raise science to the status of religious culture. He usually worked with his classes, in lectures, and otherwise for five hours every day, and it was his custom to give each month a special lecture to explain new advances in science.

Then came the year 1820, the year of his great discovery, the happiest year of his life. By what has been described as one of those lucky throws that come not twice in a thousand years to mortals, he found that an electric current can deflect a compass-needle. He reasoned that just as an electric current can produce heat and light, so it might exert magnetic influence. He had for some years possessed the germ of this idea. He convinced himself that every voltaic circuit has a magnetic field, and that the direction of movement of a magnetic needle placed near such a circuit is determined by that field. An account of his investigations, in Latin, was distributed by him to Societies and Academies in all the capitals of Europe. Praise and honours came in upon him from all sides. From the Royal Society of London he received the Copley Medal, and from the Institut of France a prize of 3000 francs.

The exact date of Oersted's discovery of electromagnetism is unknown. Arago (*Œuvres*, vol. ii. p. 50) assigns it to 1819,

but it may have been in the early spring of 1820. The announcement was dated from Copenhagen, July 21, 1820, and time

FIG. 1. OERSTED'S HOUSE IN NOERREGADE, COPENHAGEN. The original photograph was taken in 1907, shortly before the building was demolished to make way for Telephone House. The arrow indicates the room in which the first experiment on electromagnetism was performed.

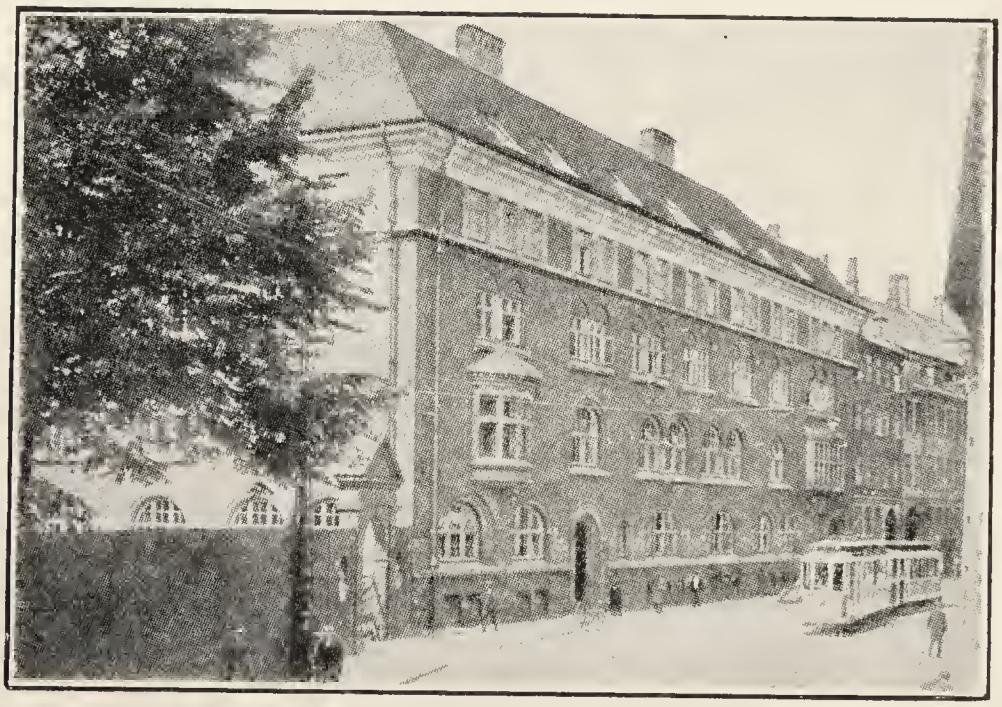


FIG. 2. TELEPHONE HOUSE, COPENHAGEN—The Headquarters of the Copenhagen Telephone Company.

must have been necessary to prepare that account. The place where the discovery was made has been carefully ascertained.

In 1819 Oersted had moved to a house, Noerregade 34 (Fig. 1), belonging to the cabinet-maker, Pingel. There for five years he remained. The house, which has since been demolished, was located at what is now the southern end of Telephone House

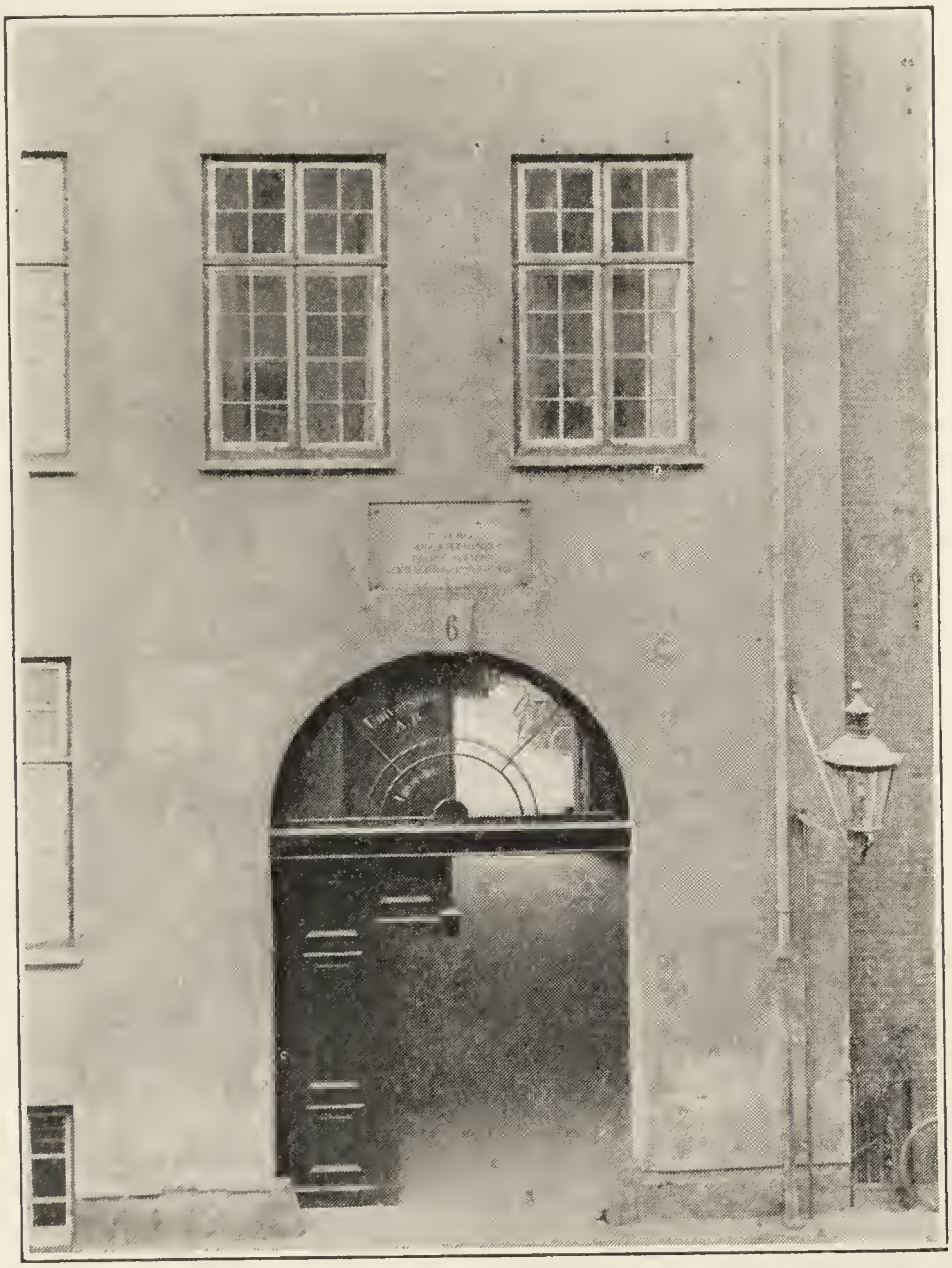


FIG. 3. ENTRANCE TO STUDIENSTRAEDE 6, an annex of Copenhagen University. The memorial plate above the doorway records that H. C. Oersted lived there as a Professor from October 1824, until his death on March 9, 1851.

(Fig. 2). It resembled a previous house there that, in common with all others in the street, was destroyed in the bombardment of 1807. Oersted rented, on account of the University, seven rooms on the third floor front and four rooms at the back for £77 a year. His private dwelling was on the second floor.

The two rooms facing Noerregade formed the lecture-room, the scene of the discovery; the others were preparation rooms. It has been possible to locate approximately the point in space,

Fig. 4. Royal Technical College, founded by H. C. Oersted in 1829. Here he lectured. It is now an annex to Copenhagen University. In the background is the tower of the Church of St. Petrie. There is a legend that to this tower Oersted extended wires from his house opposite, and installed the first electromagnetic telegraph signalling device in Denmark.

in Telephone House, where the original experiment was performed.

In 1824, Oersted departed from Noerregade 34, and occupied a Professor's house (Fig. 3), belonging to the University

at Studienstraede 6. In 1829, he founded the Polytechnic School and allowed some of the rooms of his own house to be used by that institution. At that time there was a courtyard separating his house from a private building, afterwards requisitioned by the University (Fig. 4). In 1890, for the centralization of studies in Physico-Chemistry, Inorganic Chemistry, and certain branches of Mathematics, all the polytechnic schools were transferred to a new building in Soelvgade.

Fig. 5. The Compass used in H. C. Oersted's Original Experiment on Electromagnetism. The wooden support was added later. The instrument is now in the Museum of the Physical Laboratory of Copenhagen Polytechnic Institute.

On the day of his discovery, when he had assembled the apparatus, a lecture prevented him from trying what he had in mind. At the close of the lecture, he asked his class whether they would like to observe what might happen. The class remained and witnessed the result—a deflection of the compass-needle (Fig. 5), when there was an electric current in a neighbouring wire, placed in proper relation to the needle. The movement was so small that Oersted was inclined at first to attribute it to capricious disturbances. Moreover, he was perplexed because he could not account for the fact that the movement of the

poles of the pivoted magnet, in a horizontal plane, was at right angles to the direction of the current in the wire. Why was not the line of action of the magnetism the same as that of the originating electric current? Later, he returned to the investigation; he now used a much larger battery—copper-zinc, sulphuric acid—and he obtained a decided deflection of the needle. His procedure was thus in accordance with the most cherished methods and traditions of the pioneers: to his imagination he gave scope enough to inspire him with a definite object of research, by experiment he sought in the direction of that object for new facts, and, lastly, when a clue appeared, he extended the research in each direction, towards weakness and towards strength, far beyond the limits required for a class demonstration of the phenomena.

In other investigations he studied especially the constitution and properties of water, and he developed for this research a new instrument (Fig. 6) for measuring the compression of liquids. Later he examined the pressure-volume law for air, and other substances, and he carried the pressure to the limits of the apparatus then existing. Further, he investigated the derivation of aluminium from clay, and a new method of preparing oxides of chlorine.

A third State-aided tour, in 1822–23, took him to England, France and Germany. On that occasion he had an opportunity to observe especially the progress of investigations with regard to light, and he replenished the laboratory at Copenhagen with a number of instruments. On his homeward journey he established the "Society for the Distribution of Teaching Natural Science". In 1828 he visited Norway and Berlin, where he addressed the physicists; and in 1830 he made a similar visit to Hamburg.

He next appears in 1834, discussing questions of physics at Göttingen with Gauss, whom he had met at Altona in 1827. From him he heard of the latest observations in magnetism, and of the new methods of measurement that Gauss had introduced. This concerned Oersted, for he had in mind the establishment of a magnetic observatory at Copenhagen. A few years later, he again went to Scandinavia, where he discoursed upon

subjects in a plane beyond the conventional circle of natural philosophy—he lectured upon the relation of physics to beauty, somewhat in accordance with Leibnitz's hypothesis of harmony and unity between the laws of nature and the laws of reason.

FIG. 6. H. C. OERSTED'S PIEZOMETER, now in the Museum of the Physical Laboratory of Copenhagen Polytechnic Institute. The barometer tubes inverted in mercury are seen within a glass cylinder having thick walls. The whole was filled with water or other liquid, and the pressure was applied by the piston, operated by a screw rotated by the handle.

His study of Danish literature never ceased. Into political affairs he entered boldly, and he made frequent communications to newspapers and journals, pleading for freedom of the press and for the advance of generous ideas in government. At

the other extreme, he went so far as to write in 1836 a great lyric-didactic poem entitled, "The Air Ship". It will suffice here to quote briefly from L. and J. B. Horner's English translation:

> Turn, then, and look
> Upon the varied business of man's being,
> Where the inventive spirit finds fit work
> For the free hands to do. In sooth, we might
> A thousand wonders add unto the seven
> Which the past world astonished.
>
> . . . .
>
> What Nature lent
> Her feathered children of the air—to soar
> With outspread wing, free in Heaven's azure vault—
> Art has outdone; and now majestic floats
> The dweller of the earth in regions where
> The kingly eagle has not dared to soar.
> Did not the hapless fate of Icarus
> The poet warn that such an airy flight
> Secure he cannot dare? Praised be the age
> When wonders are so rife that one like this
> Is lost among their number manifold.

Oersted, however, was not the first to derive poetic inspiration from the advance that was being made in aeronautics. The subject had long engaged the attention and had fired the imagination of men in the front rank of natural science. Twenty years earlier, Sir George Cayley had contributed a valuable article to the *Philosophical Magazine* (vol. 47, No. 214, February 1816) on aeronautics, and had concluded with a verse, at once stirring and prophetic, by Dr. Darwin:

> Soon shall thine arm, unconquer'd steam, afar
> Drag the slow barge or drive the rapid car;
> Or on wide waving wings expanded bear
> The flying chariot through the fields of air.
> Fair crews triumphant, leaning from above,
> Shall wave their fluttering kerchiefs as they move;
> Or warrior bands alarm the gaping crowd,
> And armies shrink beneath the shadowy cloud.

Behold Oersted now, Secretary of the Königl. Gesellschaft der Wissenschaften; Professor Ordinarius at Copenhagen; Corresponding Member of the Science Académie of the French Institut; Director of the Polytechnic School of Copenhagen,

which through his personal influence with Friedrich VI. he personally had called into existence; Knight of the Ehrenlegion (1837); Conferenzrat (1840); Knight of the Prussian Order *pour le mérite* in the Sciences and Arts (1842); possessor of the honorary diploma of Erlangen as Doctor of Medicine (1842), and of the Grand Cross of Dannebrog (1847). These dignities, however, did not prevent him from extending his studies, his sympathies, and his beneficent influence.

To appreciate the scope and character of the man and his achievements, it is necessary to recall the turbulent age and circumstances in which he lived. His boyhood was marked by the war of 1789, between Denmark and Sweden, by the struggle against serfdom in his country, and by a general movement towards political freedom. In the subsequent wars, Denmark—threatened by Napoleon—entered a league that brought her into conflict with England. In 1805, Napoleon overthrew Austria and Russia at Austerlitz, and formed the Confederation of the Rhine under the protection of France. This was followed in 1806 by the abdication of the Emperor Francis II., an event that marked the end of the Holy Roman Empire. The ultimate result was that when, in 1814, the Napoleonic Empire began to fall to pieces, Denmark lost possession of Norway, and suffered impoverishment and distress, followed by an interval of agitation for a free constitution—an agitation that in various phases disturbed the country to the day of Oersted's death.

To comprehend where Oersted stands in the world's history, therefore, it is essential to realize the perilous position of his country at several periods during his long life. The main trouble began in 1780, when Denmark declared the Baltic to be closed to the armed vessels of belligerent Powers. At that time England was at war with the American Colonies, and also with France and with Spain. Holland, Denmark, and the whole Baltic sought nevertheless to trade with England's enemies, in timber, tar, hemp, cordage, and provisions. England accordingly used armed vessels to search merchantmen. The Dutch held that a neutral flag should exempt from seizure. England replied by attacking the Dutch. Russia assisted merchantmen bound for French ports. Sweden and Denmark formed an

alliance to protect their trade. Denmark meanwhile kept in close touch with Russia, and, on December 7, 1800, Bonaparte joined the Russian league and devised a plan to invade India. His first move in this direction, however, was towards the commercial exclusion of English commerce from the continent of Europe. England hit back by placing an embargo on Russian, Danish, and Swedish ships in British ports, and at the same time she fitted out a fleet for the Baltic.

Denmark, supported by France, followed by placing an embargo on all British ships in her ports; she entered Hamburg and closed the Elbe to the English, she took possession of Lübeck and, aided by Prussian troops, the combined forces closed the Weser and the Ems to English vessels.

In the early spring, England restored the balance by sending her fleet through the Sound, and by attacking Copenhagen. This fleet was under the command of Sir Hyde Parker, with Vice-Admiral Nelson as second in command. Parker did not excel, but Nelson, with his accustomed skill and zeal, accomplished the task. Parker held himself in reserve with many of his ships and, thinking that Nelson would suffer a reverse, made the signal, "Discontinue the action". It was then that the famous incident occurred in which Nelson turned his blind eye to the telescope and hoisted his own battle signal, "Engage more closely". Copenhagen collapsed; Russia also gave in to England, but France did not.

The next event that closely concerned Oersted was in 1807. Bonaparte insisted that Denmark, Sweden, and Portugal should be compelled by France and Russia to enter into war against England. His object was to add the navies of Denmark, Sweden, and Portugal—about forty ships of the line—to his forces. For England it was a matter of life or death, and her immediate counter blow was to seize the Danish fleet. Admiral Gambia sailed from England on July 26, 1807, with forty-two fighting ships, and also with twenty-seven thousand troops under Lord Cathcart. Zealand was blockaded, Copenhagen was bombarded from September 2 until September 5 of that year, and the entire Danish fleet of eighteen sail of the line, ten frigates, and forty-two smaller vessels were forced to surrender.

A great misfortune fell upon H. C. Oersted in 1813 when his brother, Niels Randulph Oersted, who was an officer in the Russian army, was killed at the battle of Leipsic. The troubles of his country pursued the great philosopher to the end, for in 1848 Germany was seeking to annex part of Denmark, and the Danish army absorbed much-needed wealth. It is noteworthy that in France, in 1848, Arago was Ministre de la Guerre et de la Marine, and that Oersted, counting upon their friendship in the field of science, tried through him to influence French opinion concerning Slesvig in favour of Denmark, but without success.

It is necessary next to glance at the state of electrical knowledge as Oersted found it, and as he left it. That lightning, from a distance, could reverse the poles of a magnet, and that an electric discharge from a Leyden jar would have the same effect, was familiar to him. Benjamin Franklin, in 1749, had used these facts as an argument in proof of the identity of lightning and electricity; and "the magneticalness of lightning" had been referred to in 1756, in a history of the Royal Society. Similarly, that there was possibly an analogy between electricity and magnetism had long been a suggestion amongst physicists; for in 1767, Jan Hendrik Van Swinden, of Amsterdam, had discussed it in a prize essay and had decided that there is no definite analogy. There must be recalled also the circumstance that, in 1802, Adam Walker published *A System of Familiar Philosophy* relating to the identity of light, heat, and electricity as modifications of a single agency, and that he declared that, "we have infinite data in favour of an electromagnetic fluid".

Hence, it is not surprising that Oersted's discovery was, at first, imperfectly understood, and that there were critics who suggested that it had been anticipated. Earlier, he had himself vainly endeavoured with Ritter to trace an action between electricity and magnetism. Particulars of this investigation are to be found in the correspondence between the two physicists. To establish the priority of Oersted, it remains only to clear the issue in respect to results obtained by the Italian lawyer and mathematician Romagnosi. The case in favour of Oersted, in this instance, was established definitely by his countrymen,

Absalon Larsen, K. Prytz, and M. C. Harding. To elucidate the matter, it was desirable to dispose of the accounts of Romagnosi's results as given by Professor Giovanni Aldini in his *Essai*, published in 1804. At page 339 of the *Essai*, after describing certain developments in galvanism, Aldini directs attention, as follows, to an experiment which he ascribes to the Genoese chemist Giuseppe Mojon:

> Ayant placé horizontalement des aiguilles à coudre, très fines, et de la longueur de deux pouces, il en a mis les deux extrémités en communication avec les deux pôles d'un appareil à tasses de cent verres: au bout de vingt jours il a retiré les aiguilles un peu oxidées, mais en même temps magnétiques, avec une polarité très sensible. Cette nouvelle propriété du galvanisme a été constatée par d'autres observateurs, et dernièrement par M. Romanesi, physicien de Trent qui a reconnu que le galvanisme faisait décliner l'aiguille aimantée.
>
> (Having placed horizontally some very fine sewing needles, two inches in length, he put the two ends in communication with the two poles of a battery of 100 cells: at the end of twenty days, he removed the needles slightly oxidized, but at the same time magnetic, with a very perceptible polarity. This new property of galvanism has been established by other observers, and lastly by M. Romagnosi, a physicist of Trente, who has noticed that the galvanism causes the magnetized needle to decline).

What then was the experiment carried out by Romagnosi?

The *Manuel du Galvanisme*, by Joseph Izarne, published in Paris in 1805, contains a description of the various forms of galvanic apparatus employed up to that time for researches in physics, chemistry, and medicine. As Izarne was Professor of Physics at the Lycée Bonaparte and as he was associated with the learned societies concerned with galvanic research, his testimony may be accepted as representing enlightened contemporary knowledge of the subject. At page 120 he deals with magnetic effects of electrification, and he describes:

> APPAREIL POUR RECONNAÎTRE L'ACTION DU GALVANISME, SUR LA POLARITÉ D'UNE AIGUILLE AIMANTÉE
>
> *Préparation.*—Disposez les tiges horizontales *ab*, *bd*, de l'appareil . . . de manière que les deux boutons se trouvent à une

distance un peu moindre que la longueur des aiguilles que vous voudrez soumettre à l'expérience; et, à la place des boutons *bb*, qui sont vissés sur leur tige respective, adaptez aux tiges ou une petite pince ou bien un petit ajutage aplati.

Usage.—Après avoir placé l'aiguille de manière que ses deux extrémités soient prises dans les deux petites pinces, établissez une communication de *d*, avec une des extrémités d'un Électromoteur (Volta pile, *vide* p. 19), et de *a*, avec l'extrémité opposée.

Effets.—D'après les observations de Romagnési, physicien de Trente, l'aiguille déjà aimantée, et que l'on soumet ainsi au courant galvanique, éprouve une declinaison; et, d'après celles de J. Mojon, savant chimiste de Gênes, les aiguilles non-aimantées acquièrent, par ce moyen, une sorte de polarité magnétique.

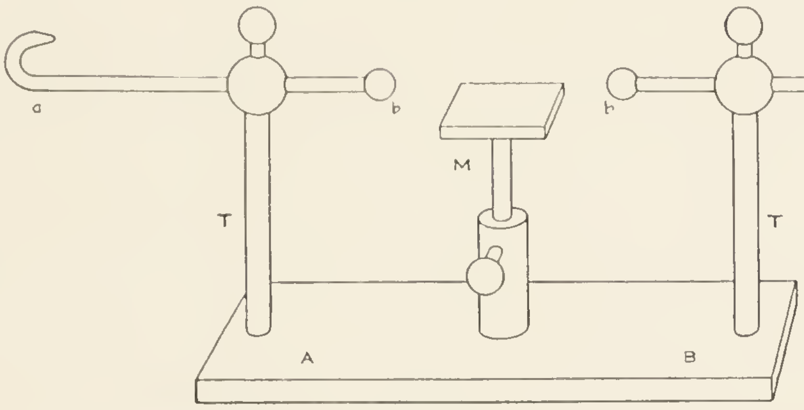


FIG. 7. APPARATUS AS USED BY ROMAGNOSI.

(APPARATUS TO SHOW THE EFFECT OF GALVANISM ON THE POLARITY OF A MAGNETIC NEEDLE

Preparation.—Arrange the horizontal rods *ab*, *bd*, of the apparatus (Fig. 7) in such a manner that the two knobs are at a distance apart a little less than the length of the needles with which it is desired to experiment; and at the knobs *bb* that are screwed to their respective rods, attach a small clip or a small flattened extension.

Manipulation.—After having placed the needle in such a manner that its two ends are held in the two small clips, establish connection from *d* with one of the terminals of the Volta pile, and from *a* with the opposite terminal.

Effects.—According to the observations of Romagnosi, a physicist of Trente, a needle already magnetized and subjected thus to a galvanic current will show a declination; and according to those of J. Mojon, a learned chemist of Genoa, non-magnetized needles acquire by this means a sort of magnetic polarity.)

Here, there is nothing in common with Oersted's experiment; for here the needles are fixed and in Oersted's case the compass needle is free to turn; here the current is passed through the needles and in Oersted's case it traverses a separate conductor.

The original account of Romagnosi's experiment appeared in a newspaper, the *Gazetta di Trento*, of August 3, 1802, and it leaves no doubt of the manner in which he carried out the experiment. Fig. 7 illustrates the support upon which the needles were placed and held fixed by clips, and the knobs by means of which the current was passed through them. The *Gazetta* account is as follows:

Senator Giandomenico Romagnosi, a resident of this city and known to the literary world through his other profound works, hastens to communicate to the physicists of Europe an experiment relating to the galvanic fluid as applied to magnetism.

Having prepared the pile of Mr. Volta, composed of round discs of copper and zinc alternating with an interposed layer of flannel moistened with water impregnated with a solution of sal ammoniac, he attached to the pile a silver wire linked at various intervals like a chain, The extreme link of the chain passed through a glass tube, from the outer end of which a silver button projected, it being connected to the said chain.

This being done, he took an ordinary magnetized needle shaped like a mariner's compass mounted at the centre of a square wooden block, and after having removed its crystal cover he placed it on top of a glass insulator in the vicinity of the said pile.

Then he took hold of the silver chain, and seizing the same by the said glass tube, he applied its end or button to the magnetic needle, and by holding it in contact for a period of a few seconds, he caused the needle to diverge some degrees from the polar direction. After the silver chain had been removed, the needle remained stationary in the diverging direction now given it. Once more he applied the same chain making the said needle diverge still more from the polar direction, and he always obtained the result that the needle remained in the position where he had left it, in such a manner that the polarity remained entirely annihilated. In order further to verify this result, he brought as near as possible to the magnetized needle (without touching it, however), now a piece of watch spring and then other iron instruments, which previously attracted the same needle forcefully at a four times greater dis-

tance, but now when exposed to the action of galvanism they were unable to move the needle even the width of a hair.

Now look how Mr. Romagnosi then proceeded to restore the polarity. With both hands he tightly gripped, between the thumb and the index finger, the extremity of the wooden insulated box without shaking it, and he held it thus for some seconds. Then the magnetized needle was seen to move slowly, and to recover its polarity, not all at once, but by successive pulsations in similar manner as a clock-hand serving to indicate seconds.

This experiment was made in the middle of May, and was repeated in the presence of some spectators. On such occasions he obtained also, without difficulty, the electric attraction at a quite appreciable distance. He made use of a fine piece of thread soaked in water saturated with sal ammoniac, and attached it to a small glass rod, and then he approached the said silver chain to the thread, to within a distance of about one line, and he saw the thread fly into contact with the button of the chain and swing up, it remaining all the time attached in a manner similar to the electric experiments.

From this evidence it is manifest that Romagnosi was concerned solely with the effect of electrical discharges through the magnets themselves, and that he repeated, in effect, the experiment of Mojon, by sending a more or less continuous current through the magnets. As his magnets were fixed, he did not observe the rotary movement that was the essence of Oersted's discovery. Moreover, Romagnosi himself made no claim to such a discovery. Romagnosi observed the more or less permanent magnetizing or demagnetizing effect of a current through the magnets—the annihilation of polarity. Oersted did not send a current through magnets, but through a separate conductor. Consequently the novelty of Oersted's experiment may now be regarded as established beyond controversy.

The Copenhagen experiment gave to research an impulse as far-reaching as it was momentous. The account of it arrived in Paris from Switzerland, and at the meeting of the *Académie* on September 11, 1820, an academician who had just arrived from Geneva repeated it with great success. Seven days later, on September 18, Ampère made known his grand generalization concerning it. On September 25, Arago described, at the Bureau des Longitudes, experiments in which a current from a voltaic

pile, when conveyed through a conductor, is caused to magnetize rods of soft iron separate from the conductor. On November 16, at a meeting of the Royal Society, Sir Humphry Davy read a paper on the magnetizing influence of galvanism on bars of steel (*Philosophical Magazine*, vol. 56, 1820, pp. 381-2). The experiments were made in the laboratory of the Royal Institution. The batteries were twelve troughs of four-inch plates, mounted with double coppers "agreeable to Dr. Wollaston's plan". In the same journal, at p. 394, is an account of a further result, attributed to Oersted, and explained as follows:

A plate of zinc (about three inches high and four inches broad) placed in, and by an arch of small wire connected with, a trough nearly fitting it, made of thin copper and containing a mixture of one part of sulphuric acid, one part of nitric acid, and 60 parts of water, forms an apparatus which being suspended by a very small wire (only sufficiently strong to bear its weight) will, if a powerful magnet be presented to it, exhibit magnetic polarity—turning its corresponding pole to the pole of the magnet. The suspending wire is attached to the apparatus by a thread rising from one side of the trough to the wire, and descending to the other side of the trough; and the plate of zinc is kept from coming in contact with the copper case by a piece of cork interposed on each side of the plate.

The *Philosophical Magazine*, vol. 57, 1821, pp. 40-49, contains a paper by Hatchett on the electromagnetic experiments of Oersted and Ampère, in which Oersted is said to have opened up a new field to the inquiries of philosophers, for—

It is to him that we owe that fine observation that a metallic wire which communicates with the two extremities of a Voltaic electrical apparatus acquires the very remarkable property of acting at a distance on a magnetic needle. This metallic wire has been named the conjunctive wire.

Hatchett states that it had for some time been known that conjunctive wires may become heated, redden, and burn in atmospheric air, and he emphasizes the fact that—

For twenty-three years the electric piles of Volta had been in use, and no philosopher had yet thought of bringing a magnetic needle near one of these piles in action. This inspiration was reserved to Oersted; and it must be confessed that chance had much

less share in it than in many discoveries with which physical science has been enriched.

Oersted had long before written a memoir with the title: *An Inquiry into the Identity of Chemical and Electrical Forces*, and this book was translated and published in 1807. In that inquiry he sought for proofs that magnetic and electric forces are identical, but he found none. Hatchett remarks upon the curious circumstance that when Oersted discovered the action of the conjunctive wire upon the needle he explained this new phenomenon by a hypothesis which supposes that the negative electricity acts only on the northern pole of the needle, and that the positive electricity acts only on the southern—*Annales de Chemie*, August 1820, p. 244—implying a total difference between electric and magnetic fluids, since the magnetic fluid, whether considered as positive or negative, ought to act equally on both poles.

To demonstrate, by experiment, the identity in question, it was necessary to explain all the phenomena—the mutual action of two magnets, the action of a conjunctive wire upon a needle —"without admitting in a magnet any other fluid than the acting electric fluid". This task was undertaken by Ampère. The inter-relationship of electricity and magnetism was further established by Arago who observed that steel needles, placed within spirals of wire, became magnetized when electric discharges were passed through the wire from static electrical machines, or when currents were passed through them from Voltaic batteries.

In the discovery of aluminium, Oersted played a great part, but to Wöhler he had to leave the honour of producing the metal in a pure state. Wöhler, however, recognized the value of Oersted's work in this direction.

Oersted's view was that, given diligence and a healthy brain, advances in natural science can be made even by those of but limited education. It was in accordance with principles of this character that he proposed, in 1824, and founded in 1829, the Selskabet for Naturlaerens Udbredelse—the Society for the Popularization of Natural Science. This he did by giving lectures in the towns, by the distribution of pamphlets, and by

supporting those who desired to improve their acquaintance with technical science. The Society still flourishes, and in memory of its founder it has established an Oersted medal as a prize. He realized that the inculcation of scientific principles governing the operations upon which men and women are employed, is a better mission for such a Society than the attempt in schools to teach manual work. He was not an advanced mathematician, but he recognized the necessity for having close enough acquaintance with the subject to enable the results of investigations to be reduced to numerical terms. His dominating idea was the interrelation between what at that time were called the various forces of the universe; and he saw that the future welfare of his country must depend upon the development of inventive faculties, directed towards the utilization of natural resources. His plea, therefore, was for technical education strongly supported by natural science. In his wanderings over a wide field, he was led at last by his philosophy into close touch with the basic problems of humanity. Thus he learnt to see life steadily, as well as sympathetically, and to see it whole. His influence upon contemporary thought and action finds testimony in Hans Christian Andersen, who confessed to the support and encouragement he received from him.

In 1846, Oersted again visited England, and he was present at the meeting of the British Association at Southampton in that year. The remarks, on that occasion, of Sir John Herschel convey an idea of the impression created upon Englishmen by Oersted, and by his discoveries:

> To look at his calm manner, who could think that he wielded such an intense power, capable of altering the whole status of science, and almost convulsing the knowledge of the world . . . (his discoveries) went almost to the extent of obliging them to alter their views on the most ordinary laws of force and motion. . . . The electric telegraph, and other wonders of modern science, were but mere effervescences from the surface of this recondite discovery, which Oersted liberated, and which was yet to burst with all its mighty force upon the world.

The fiftieth anniversary of his association with Copenhagen University was on November 7, 1850. At that festival he

was greeted by all Denmark, from the King to the most lowly of the people. The Danish Government presented him with a country residence, Fasangaarden in Frederiksberg Park, precious to his memory as the former home of his friend Oehlenschläger. Students in torch-light procession sang verses composed in his honour, and men and women of all ranks and opinions joined with one accord in the festivities. He was then seventy-three years of age, and was actively occupied with lectures and with literature.

During that winter, he busied himself with preparations for the transfer to Fasangaarden; but he never occupied it. He caught cold, and, after a brief illness, died on March 9, 1851.

He was below medium height, of open countenance, of florid complexion, somewhat stout, in manners kindly, by nature gracious, loyal to his King, devoted to his country and to the cause of humanity. In his scientific work he was often baffled but never discouraged, his perseverance helped him to the end. Towards the end, he was able to write to a friend the secret of his life:

> In my family I am as happy as a man can be. I have a wife whom I love, and children who are dear to me and who prosper. I have three sons—of whom one is of age and is employed in the forestry service of the King—and four daughters, of whom the eldest three are either married or betrothed. My brother, who for some time was a Commissioner for the King in our provincial parliament, has recently become a Minister of State. As for me, I am still a professor and director of the Polytechnic School and Secretary of the Royal Society of Sciences.

Oersted thus had three sons and four daughters. The eldest daughter, Karen, married E. A. Sharling, Professor of Chemistry at Copenhagen. Another daughter, Marie, married S. N. P. Hasle, who for forty-one years was pastor at Odsherred in Zealand. The youngest, Matilda, remained to cherish her father's old friend Hans Christian Andersen, and to her Andersen bequeathed the manuscripts of his own stories. Oersted's sister, Barbara Albertine, married Georg Jacob Bull, who became President of the Supreme Court of Norway.

His dearest friend, Adam Gottlieb Oehlenschläger (1779–

1850) also joined the immortals; with his poems and his sagas he became the great minstrel of the North, who sustained his countrymen with hope and courage in the midst of their tribulations.

In 1920, the Foundation Carlsberg published, under the editorship of Mr. M. C. Harding of the Polyteknisk Laereanstalt, Copenhagen, a collection of Danish, French, German, and a few English letters, representing some of the correspondence of H. C. Oersted with men of science of his time. Mr. Harding has dealt with these documents with such care and precision that they now constitute the best possible means both of judging of the scope and character of Oersted's opinions, friendships, qualities, and achievements, and of observing the variety of ways in which his influence was exerted. From them it appears that, at the threshold of his professional life, Oersted was regarded by some of his scientific acquaintances as *un exalté*, and too empirical, and as something of a dreamer. In 1820, however, when one of his dreams came true, most of his friends —and even the exacting Berzelius—were quick to acknowledge the substantial character of the stuff of which the dreams were made. Erman recognized the discovery of electromagnetism at once at its true worth. Seebeck, on the first verbal account of it, shook his head, but when he read the complete communication he offered "a thousand warmest greetings", and said it deserved a prize like that given by Bonaparte to Volta. Arago, who probably had been acquainted with Oersted for a few years before the announcement of the great experiment, also at first entertained doubts, but became convinced and was the first person in France to direct the attention of scientific men to it. To Switzerland, and to De la Rive, however, belong the credit of the first public demonstration. In England, on November 16, 1820, at the Royal Society, Davy assigned to Oersted the honour of priority in the discovery of electromagnetism, and declared that no anterior experiments by others could change the fact of that priority. His opinion of Oersted is also recorded —"He is chiefly distinguished by his discovery of electromagnetism. He is a man of simple manners, of no pretensions, and not of extensive resources; but ingenious, and a little of a

German metaphysician." Faraday for once confessed that chance counted for something; but he was convinced of the originality of Oersted's experiment. He said of Oersted: "His constancy in the pursuit of his subject, both by reasoning and experiment, was well rewarded in the winter of 1819 by the discovery of a fact of which not a single person beside himself (Oersted) had the slightest suspicion, but which, when once known, instantly drew the attention of all who were able to appreciate its importance and value."

Oersted and Faraday met for the first time in London in 1823. Owing to the subordinate position of Faraday to Davy, Oersted could not then get into direct touch with him. It was therefore under conduct of Davy that Oersted took part in some experiments of Faraday, that year, at the Royal Institution. In 1846, when Oersted for the second time visited Faraday's laboratory, his attention was given to the great horse-shoe electromagnet. At that time Faraday was not present, but his old laboratory-assistant acted as guide. After the 1846 British Association Meeting, however, Faraday himself showed Oersted his experiments on diamagnetism. Upon his return to Copenhagen, Oersted constructed an electromagnet (Fig. 8) which is still preserved in the Polytechnic School. With it, in 1847, he demonstrated Faraday's experiment in diamagnetism at the Royal Académie of Copenhagen, and he continued that work until 1849. The two philosophers followed closely the accounts of one another's researches on such subjects as compressibility, the validity of Mariotte's law, the liquefaction of gases, and the formation of sonorous figures on vibrating plates.

Of all the congratulations he received from men of scientific perception, the most emphatic were those of Dr. Thomas Young, who spoke of the marvellous discovery that elevated Denmark to a rank in science which it had not held since the days of Tycho Brahe. In Germany, the learned Dr. J. S. C. Schweigger paid no less a tribute when he declared that Oersted's experiments in magnetism were the most interesting that had been carried out in that domain of science for a thousand years. Concerning telegraphy Schweigger, in 1848, wrote to Oersted:

Sie waren es, mein verehrter Freund, dessen Elektromagnetismus die engste Verbindung der Menschen herbeigeführt durch die nun so zahlreich angelegten electromagnetischen Telegraphen. Statt sich dieser engen Verbindung zu freuen, fangt man Kriege an

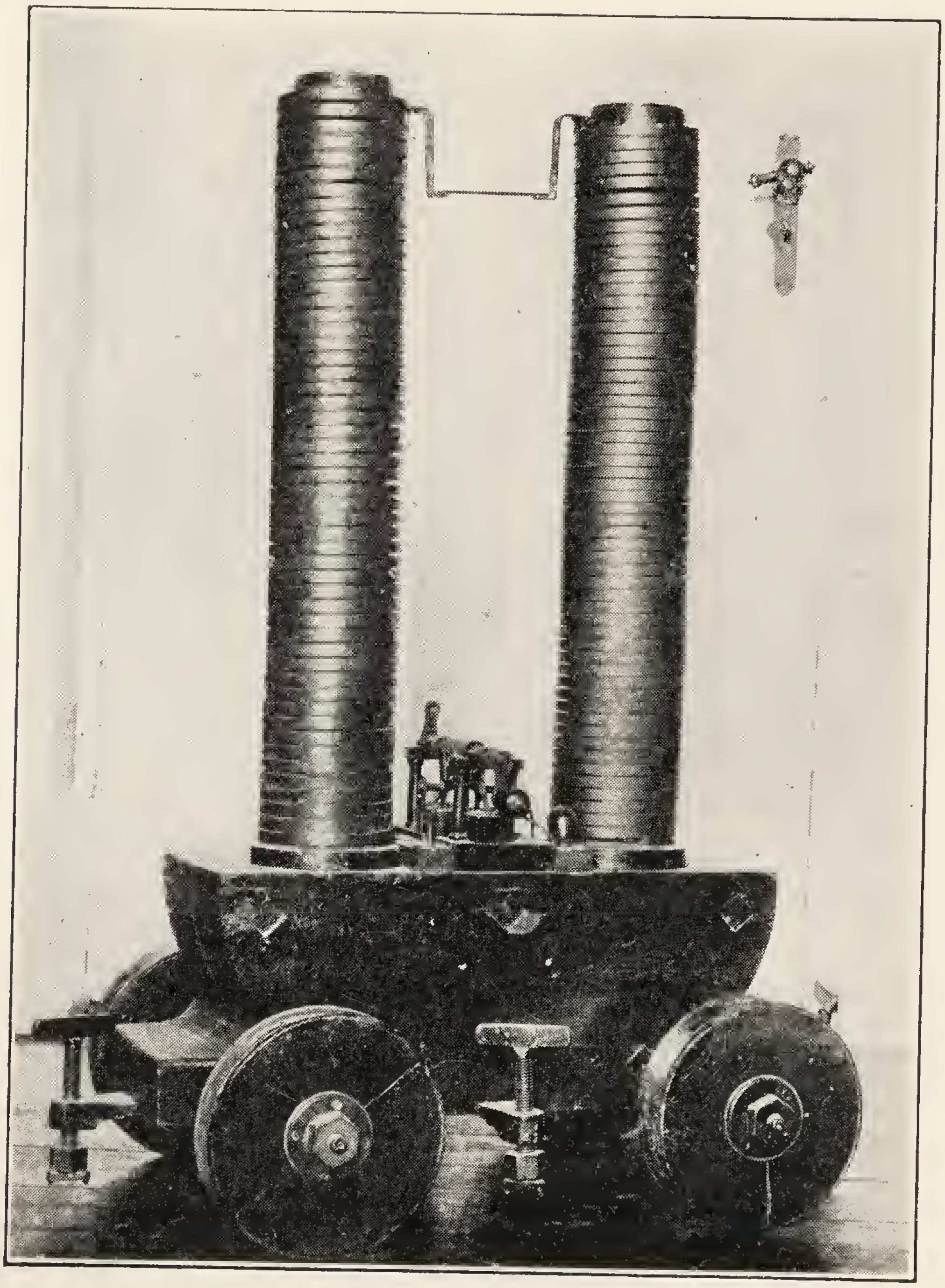


Fig. 8. H. C. Oersted's Electromagnet, constructed after he had seen Faraday's Electromagnet. The height of the magnet cores was about 50 centimetres.

und entzweit selbst die durch Abstammung, Sprache und Sitte von Natur befreundeten Völker, wie Dänen und Deutsche.

(It was you, my honoured friend, whose electromagnetism made the close bond between men, by means of the now numerous forms of electromagnetic telegraphy. But instead of this bond being a cause for rejoicing, it has begun to be a means of war and separa-

tion of languages and customs of friendly peoples like the Danes and the Germans).

The first in America to emphasize the importance of Oersted's discovery of electromagnetism was Joseph Henry (*Transactions of the Albany Institute*, vol. 1, pp. 22-24, 1830). By 1841 the name of Oersted was as well known in America as in any part of Europe.

The impression created by the researches of Oersted is that his greatest successes were derived from experiment. Even in 1801 he was seeking to improve his powers as an experimenter by learning glass-blowing in Leipsic; and, a year or two afterwards, he was at Haarlem repeating Ritter's experiments in company with Van Marum. In 1846 he witnessed with enthusiasm the experiments of Faraday. It is curious to observe, therefore, that in 1849, when he was approaching the last stage of his career, he confessed, in a letter to Sir John F. W. Herschel, that he had continuously turned his mind against the results which could be drawn from the experimental sciences to those which are commonly treated as depending particularly upon mental powers. He added: "I am far from approving the German metaphysics. . . . I have long since been led to form a philosophy of my own . . . through the corporal world to the mental." The truth is that like all who seek earnestly to penetrate the laws of Nature, he developed a mind maintained in equilibrium by perpetual spin and oscillation between fact and opinion, experiment and enlightened imagination.

It is in his friendship for Ritter that his finest qualities appear. He met him first in 1801 when Ritter's work was little known. In March, 1803, in Paris, Oersted gave a demonstration of Ritter's invisible (ultra-violet) rays that could act chemically, and in May of that year he did the like for Ritter's secondary cell. Biot thereupon asked Oersted to obtain from Ritter a direct communication upon these subjects, and it was because of it that Ritter, obscure, reserved, eccentric, and impoverished, obtained the prize of 3000 livres. Ritter in 1802 was working every day for months from 9 A.M. to 10 P.M. with the Duke of Gotha's mammoth battery of 600 pairs of plates. Chemistry, he truly said, takes a long time. His experiments with a zinc-

silver needle on a pivot led him into strange fancies, and his magnetic battery, consisting of a series of watch-glasses containing water and united each to the next by a series of bent magnetized steel rods, must have puzzled even Humboldt. Ritter promised himself a "recht magnetischen Winter", but he became involved instead in an assemblage of paradoxical phenomena concerning which he appealed constantly to Oersted. Then he visited Italy, and kept Oersted informed of what was happening there with regard to alleged animal "magnetism", water-finding, mineral-finding, and other pseudo-scientific matters, the results either of deception or of lack of precise means of analysis. In due course came the battle of Austerlitz. Ritter wrote to Oersted on December 8, 1805, telling him of the losses sustained, and of his privations.

München schickt sich unterdessen immermehr zu einem Kriegstheater an, weil hier ein Hauptübergang über die Isar ist. In wenig Tagen sind wir höchst wahrscheinlich den Kuglen des Geschützes ausgesetzt. Die franz. Vorposten sollen bereits in Augsburg seyn.

(Munich is meanwhile becoming more and more a theatre of war, because here there is an important crossing of the Isar. In a few days we shall probably be blown sky-high by the cannon-balls of the artillery. The French advanced troops are already at Augsburg).

"Thank God," he added, "experiments don't cost much." His highest hope was that Napoleon would visit the Akademie at Munich—the Empress Josephine was already in that city. This aspiration concluded—

Von meiner Seite soll er dann hier sehen, was er in Bologna bloss gehört hatte. Lebe wohl.

Ritter told Oersted that he desired to talk to Volta about the possible electrical origin of animal magnetism, but

Gegen Leute, wie Er, muss man höflich seyn, u. nach ihrer Sprache bequemen.

(With people like HIM we must be polite, and mind what we say.)

Behind all this there was Ritter's poverty and his endeavour to find relief by winning the French prize. He wrote piteously to Oersted for assistance:

Geld ist das was mir fehlt, u. was mir mehr fehlt, als es nur je gefehlt hat;

and reading between the lines of this correspondence it becomes obvious that Oersted aided him with his accustomed generosity. On the other hand, Oersted absorbed some important home truths from Ritter, and especially to temper existence with harmony and colour. Ritter chaffed him about the rigidity of his philosophy, and added:

Vergiss nicht, dass wir Künstler seyn sollen. Kunst aber brauche ich dir nicht zu definiren.

(Don't forget that we are artists—but I need not define art for you.)

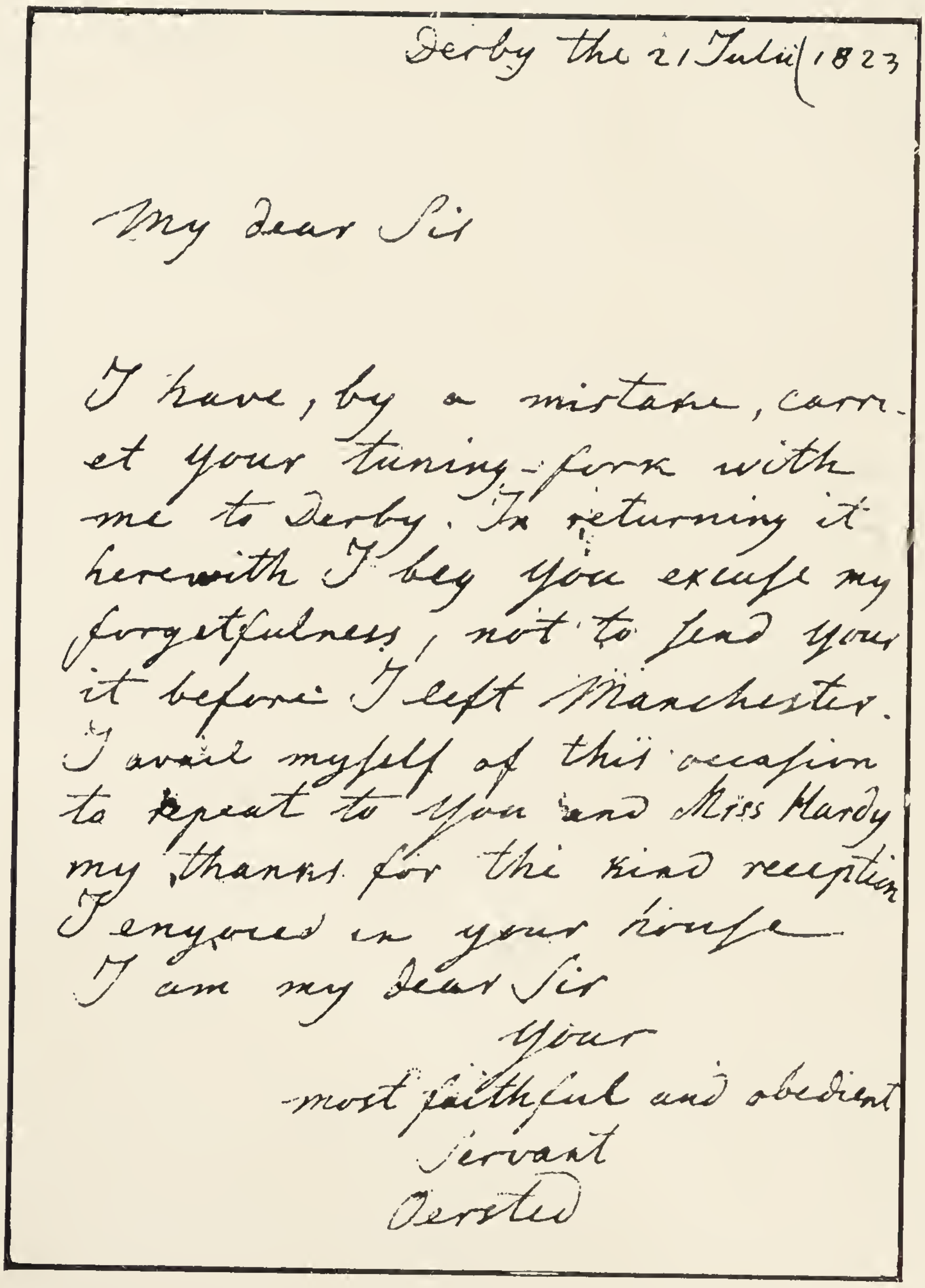
Ritter was perplexed about magnetism; it seemed to him to belong to the other side of Nature and to lead to the underworld. Its laws appeared to be opposed to all ordinary laws. He hoped, however, some day to resolve doubts by raising all metals to the magnetic rank of iron, possibly by extreme cold or by extreme heat.

The speculations of Winterl, in 1800, concerning chemical action, made upon Oersted a profound impression. It was Winterl who introduced the two subtle conceptions Andonia and Thelyke—elements more simple than all others, but entering into all. Oersted was captivated by the idea, but he found at Berlin a "terrible prejudice" against it, for Andonia and Thelyke were there denounced as phantoms. From such airy nothings, however, was spun thus early his imaginative faculty, and, above all, the enthusiasm that carried him unscathed across the turbulence of his time.

By his travels and by his close study of contemporary scientific writings, Oersted kept himself informed of progress and utilized his knowledge for the good of his country. He could not always hasten the advance as much as he wished. For example, he was aware in 1811 of the existence of gas lighting (thermo-lampe)[1] but although it was in evidence in

[1] William Murdoch's first trial of coal-gas, in his home at Redruth, was in 1779. His first installation of gas lighting was at the works of Phillips and Lee, of Manchester, in 1807.

London in 1812, Stockholm waited for it until 1853, and Copenhagen until 1857. After a visit to Gauss at Göttingen in the summer of 1834, he established a magnetic laboratory in a wooden shed in the garden of the Polytechnic School at Copenhagen, and later upon the ramparts of that city. He was a



Derby the 21 Julii 1823

My dear Sir

I have, by a mistake, carri-
et your tuning-fork with
me to Derby. In returning it
herewith I beg you excuse my
forgetfulness, not to send you
it before I left Manchester.
I avail myself of this occasion
to repeat to you and Miss Hardy
my thanks for the kind reception
I enjoyed in your house
I am my dear Sir
Your
most faithful and obedient
servant
Oersted

FIG. 9. SPECIMEN OF H. C. OERSTED'S HANDWRITING. It is a copy of a letter sent to a Mr. Hardy when H. C. Oersted was in England in 1823.

friend of Hansteen, and on his journeys through Europe he introduced Hansteen's apparatus for the observation of terrestrial magnetism. It was in fact at the instigation of Oersted that Arago used it in Paris.

The chief object of his journey in 1822–23 was to discuss optics—with Seebeck in Berlin, Frauenhofer at Munich, Biot,

Fresnel, and Arago in Paris, Wollaston, Young, and Herschel in London, and Brewster in Edinburgh. Another matter investigated was whether, in accordance with Halley, the earth had four magnetic poles, or whether, as propounded by Euler, it possessed only two, and those unequal. Other puzzles were the causes of magnetic variation and of the polar lights. There was also under consideration the alleged observation by Maschmann that the crystallization of silver from the solution of the salt is more effective in the magnetic meridian. Later, like most of the pioneers, Oersted directed his attention to the atomic theory, to the dynamics of space and to cohesion.

In 1839, Christian VIII (1786–1848) succeeded his uncle upon the throne of Denmark. He set the finances in order and in general he ruled well. In 1842 he became president of the Royal Society of Sciences at Copenhagen, of which Oersted was secretary. Occasionally the King took the chair at the meetings, but in any case it was Oersted's duty to hand to him a report of all that transpired, and this brought the philosopher and the monarch into constant touch—a circumstance that proved of advantage to the cause of natural science in Denmark. It also facilitated the bestowal of honours. Sir John F. W. Herschel, who became a Knight Commander of Dannebrog, was not allowed to wear the Order in England. This, however, did not deter Sir Roderick Murchison from entering the lists. His birthplace was Scotland, and in reply to preliminary interrogatories he said that if His Majesty, King Christian, should be pleased to confer any Order on him, it would not be necessary for the case to be tried as he would simply wear the Order. And he did.

Oersted's opinion on the broad issue was: "I cannot but feel the ludicrous in all these titles and distinctions, but I am somewhat reconciled to them by the consideration that they seem to counterbalance the aristocracy of birth and of money. Some day to come will bring a better balance between the honours and merits than this, but I apprehend that this day is not very near."

The friendship between Oersted and Sir David Brewster began at the end of June, 1823, at Edinburgh, where Brewster

introduced him to Sir Walter Scott and to other celebrities. In consequence of this visit, Oersted wrote several articles for the *Edinburgh Encyclopaedia*. Brewster and Oersted were together for the last time at the meeting of the British Association at

FIG. 10. H. C. OERSTED, from an Engraving by A. Weger, in Leipsic.

Southampton in September, 1846. The financial position in Denmark was then serious. Oersted probably lost heavily, and was anxious to restore his income by the sale of his books and writings in England. Yet he always had entertained a poor opinion of publishers—in 1803 he described them as unhappy mortals who understand nothing of the contents of what is

offered to them and who depend solely upon the name of the writer. This opinion was strengthened when Mr. Longman refused to publish an English edition of his book on the Philosophy of Beauty. In Danish, German, and French he was a concise writer and prided himself upon the quality of his productions. In English he was fluent but less perfect. Writing, in any case, was not a pleasure to him. "I write", he said, "as if I had to pay for every line. I know some men who write as if every line were to be paid for by others"; and he confessed to "a singular disinclination to writing letters, which has often excited my own astonishment". Nevertheless in 1805 he contemplated writing a Book of Physics for Ladies, and he would have done it if Ritter had not dissuaded him by ridicule.

For Charles Wheatstone, Oersted had considerable regard. The two men had much in common. It is stated by Mr. M. C. Harding that, in 1823, on the occasion of Oersted's visit to London, Wheatstone had just opened a musical-instrument shop where he carried out acoustic experiments so much appreciated by the Danish physicist that Oersted himself introduced Wheatstone to Sir John Herschel and to Babbage. Moreover, it was Oersted who, in 1823, made known in Paris the acoustic and other experiments of Wheatstone; and it was to Oersted that, on May 20, 1839, Wheatstone wrote from King's College, London, of

> . . . the way in which I have applied your beautiful discovery for the purpose of transmitting instantaneously, both visibly and audibly, to great distances. The first Electrical Telegraph was established by Mr. Cooke and myself on the London and Birmingham Railway in the year 1837, and we have now a line in action 14 miles in length on the Great Western Railway between London and Bristol.

And on August 16, 1844, from the same address:

> The Electric Telegraph is being brought into extensive use. Our Government has just decided on establishing a line between London and Portsmouth and a commencement is being made in France on the Paris and Orleans Railway.

Miss Petraea Sharling, the gifted daughter of Professor

Sharling, has kindly furnished from memory some of the personal details for the account of her grandfather. To the late Mr. M. C. Harding, and to other members of the Administration of Copenhagen University, thanks are due for many other details, and for kind assistance in obtaining illustrations.

N

G. S. Ohm, from a Bust by Rümann.

VII

GEORG SIMON OHM

A CENTURY ago, the science and practice of electrical measurement and the principles of design for electrical instruments scarcely existed. With a few exceptions, ill-defined expressions relating to quantity and intensity, combined with immature ideas of conductivity and derived circuits, retarded the progress of quantitative electrical investigations. Yet, amidst this confusion, a discovery had been made that was destined to create order out of chaos, to convert electrical measurement into the most precise of all physical operations, and to aid almost every other branch of quantitative research. This discovery resulted from the arduous labours of Georg Simon Ohm.

So completely has his work now merged into general knowledge, that his life is lost sight of in a law, and his name in a unit. Writings, all too brief, of his friends Bauernfeind and Mann enable some of the scattered details of his personal history to be ascertained. Relics of his laboratory apparatus, few as they are, give hints of the circumstances in which he carried out his researches. Fortunately, however, in contrast with the broken narrative that tells of his career, there exist his published scientific memoirs, collected with such care and comprehension by Eugene Lommel, that Ohm's achievements are established more firmly than they might have been if every detail of the sombre history of his honourable life had run the gauntlet of the cyclopaedias. To these memoirs must be added the volume of reprints of certain of his letters and other documents which, thanks to the industry and zeal of Ludwig Hartmann of Munich, were collected and printed as a tribute to

Ohm on the occasion, in 1927, of the centenary of the publication of the immortal treatise on the electric circuit.

Ohm belonged to a German burgher family, from father to son, locksmiths. His great grandfather was Wilhelm Ohm, of Westerholt, near Münster, in Westphalia. His grandfather was Johann Vincentius Ohm, a journeyman locksmith, who settled first at Cadolzburg; there he married, but in 1764 he made his home in the university town of Erlangen, Bavaria, where he obtained citizen rights. Johann Vincentius had two sons. The elder, Johann Wolfgang, born in 1752, was apprenticed as a locksmith in 1776, and after ten years of wandering as a journeyman he returned to Erlangen, where, in 1785, he became a master locksmith. On January 24, 1786, this Johann Wolfgang Ohm married Fraulein Beck, or Beckin, the daughter of a tailor. They had seven children. The first child of this marriage was Georg Simon Ohm, who, according to the most trustworthy authorities, was born on March 16, 1789. A second son, Martin (junior) was born in 1792. In the late summer of 1799, when Georg was but ten years old, their mother died. Of the children, only three grew up; these were Georg Simon, Martin (junior), and Elizabeth Barbara. Martin (junior), it must suffice here to observe, became a distinguished mathematician, and a Professor of Mathematics at the Military College, Berlin.

The younger son of Johann Vincentius Ohm was Martin (senior), born in 1763, *i.e.* a year before the settlement of Johann Vincentius in Erlangen. This Martin (senior) similarly became a locksmith in Erlangen; he married on February 23, 1789, at the Neustädter Church at Erlangen, Elizabeth Sabina Krug, the daughter of a peasant from the Uehlfeld district. They had five children, none of whom survived infancy. The death of his wife soon followed. On June 23, 1800, he married Sabina Katherina Frasz, a hosier's daughter, and on February 16, 1801, a daughter was born. Martin (senior) Ohm survived the birth of this daughter only a few weeks. He died on April 5, 1801, at the age of thirty-seven years, seven months, and twenty-three days. He was godfather to his nephew Martin (junior), the son of Johann Wolfgang Ohm.

These details help to dispel doubts concerning the birth-place and the dates appertaining to Georg. It is remarkable that the records of one who devoted his whole life to precision should call for so much hesitancy in acceptance, but the tablet upon the house where he is alleged to have been born, and the in-

Portrait of G. S. Ohm, from a Bust by Rümann.

scription upon his tombstone, are discordant with his history. Moreover, he could scarcely call his name his own. The date and place of birth have recently been investigated by Dr. Deuerlein of Erlangen. His account of the matter was published in a supplement to the *Erlanger Neueste Nachrichten* of June 25, 1927. He found, in the Register of the Evangelical Lutheran Church of Erlangen, Neustadt, the entry of baptism:

1789 Martius den 18 t. (wurde getauft) Johann Simon, Mstrs. Johann Wolfgang Ohms, Bürgers und Schlossers dahier, und seiner Ehefrau Maria Elisabetha geb. Beckin von hier. Söhnl. geb. den 16. Abends um 3 Uhr. Gev. war Mstr. Johann Simon Beck, Bürger und Schneider dahier, der Kindbetterin Bruder.

[1789 March 18th (was christened) Johann Simon—son of Johann Wolfgang Ohm—citizen and locksmith of this district—and of his wedded wife Maria Elizabetha, formerly Beckin of this district. The child was born on the 16th at 3 o'clock in the afternoon. Godfather was Mr. Johann Simon Beck, citizen and tailor of this district, brother of the child's mother].

This leaves no doubt that the date of birth was March 16, 1789, and that the boy was christened Johann Simon. The name by which he was subsequently known was Georg Simon—possibly to avoid its being mistaken in the family for that of his father, Johann.

The house in Erlangen upon which the memorial tablet is placed is No. 6 Fahrstrasse (Fig. 1). From the town records it is known that this house was built in 1733. Its first occupant was the builder of it, Joh. Gg. Held, who sold it to the hatter, Leonard Hofmann. From October, 1754 it was owned by the tobacconist, Andreas Wölckel. He died and left a widow who on March 1, 1779, transferred it to Elias Reinhard, a hosier, from whom it passed by inheritance on June 18, 1782, to Johann Georg Bauer, a locksmith. Two years later it was sold to Johann Friedrich Schwarz, a master white-washer. On March 31, 1791, it was sold to Johann Melchior Günther, a furniture maker, and the Günthers retained it into the nineteenth century. Consequently, No. 6 Fahrstrasse was never possessed by the family of Ohm. There is no evidence that they ever entered it.

The investigation next turns to No. 11 Fahrstrasse (Fig. 2), and to No. 20 Friedrichstrasse. No. 11 Fahrstrasse was built in 1724. It passed into possession of Link, the hosier, and it was sold by him on September 7, 1790, to Johann Wolfgang Ohm who there resided and established a locksmith's workshop. No. 20 Friedrichstrasse was built in 1719, and in the middle of the eighteenth century it was owned by Leonard Heinrich Kühn, a tailor. In 1799 it was owned by the master locksmith Johann

Vincentius Ohm. On May 3, 1801, after his death, it was taken over by his son Johann Wolfgang—the father of Georg and Martin (junior). As Martin (junior) was born on May 6, 1792, it must be concluded that his birth took place at No. 11 Fahrstrasse—not at No. 6 Fahrstrasse.

Fig. 1. No. 6 Fahrstrasse, Erlangen.

Georg Simon was born on March 16, 1789—where, nobody knows, for the residence of his parents before September 7, 1790, has not been traced. His sister, Elizabeth Barbara, born on July 24, 1794, married on June 7, 1824, the locksmith Konrad Fichtbauer (or Füchtbauer). This worthy man appears to have entered whole-heartedly into the matrimonial contract; for with Elizabeth Barbara he took over the Ohm locksmith workshop and the two Ohm houses—No. 11 Fahrstrasse and

No. 20 Friedrichstrasse. They had a son who inherited scientific propensities. He became Dr. Füchtbauer, chief member of the educational council and Rector of the Nuremberg Industrial College. By the courtesy of the Director of the Deutsches Museum, Munich, relics of some of the original apparatus of

Fig. 2. No. 11 Fahrstrasse, Erlangen.

Georg Simon Ohm are here illustrated, Figs. 3 to 7 inclusive; and it is to be remarked that these relics were acquired by the Museum from the Füchtbauer family of Nuremberg, in October, 1904.

In view of these investigations, the words on the stone tablet above the portal of No. 6 Fahrstrasse (Fig. 1), may require amendment. They read:

Georg Simon Ohm Physiker Hier geb. 16. III. 1789. Martin Ohm Mathematiker geb. 6. V. 1792

[George Simon Ohm
Physicist
Born here. 16. III. 1789.
Martin Ohm
Mathematician
born 6. V. 1792]

The house also bears a notice explaining that upon the premises feather beds are cleaned by steam and by electrically driven machinery. This association of the house with the great electrician constitutes the whole of the evidence.

The father of Georg and Martin was a man of exceptional ability. In his wanderings as a locksmith, he had studied philosophy and mathematics. This had brought him into touch with Professor Langsdorff of Heidelberg who had gone there from Erlangen. Under their father's guidance, the motherless lads made progress, and the attention of Langsdorff was directed to their aptitude. He predicted that history would repeat itself in them as a pair of brothers Bernouilli. Subsequent events accorded with his prophecy. Stimulated by this encouragement, the father decided to give them University education, upon the understanding that they must apply themselves also to become skilled locksmiths.

For one year Georg attended the Gymnasium of Erlangen. At Easter, 1805, he entered Erlangen University (Fig. 8); on May 3, 1805, he matriculated in Philosophy. His studies were immediately directed to mathematics and physics. Unfortun-

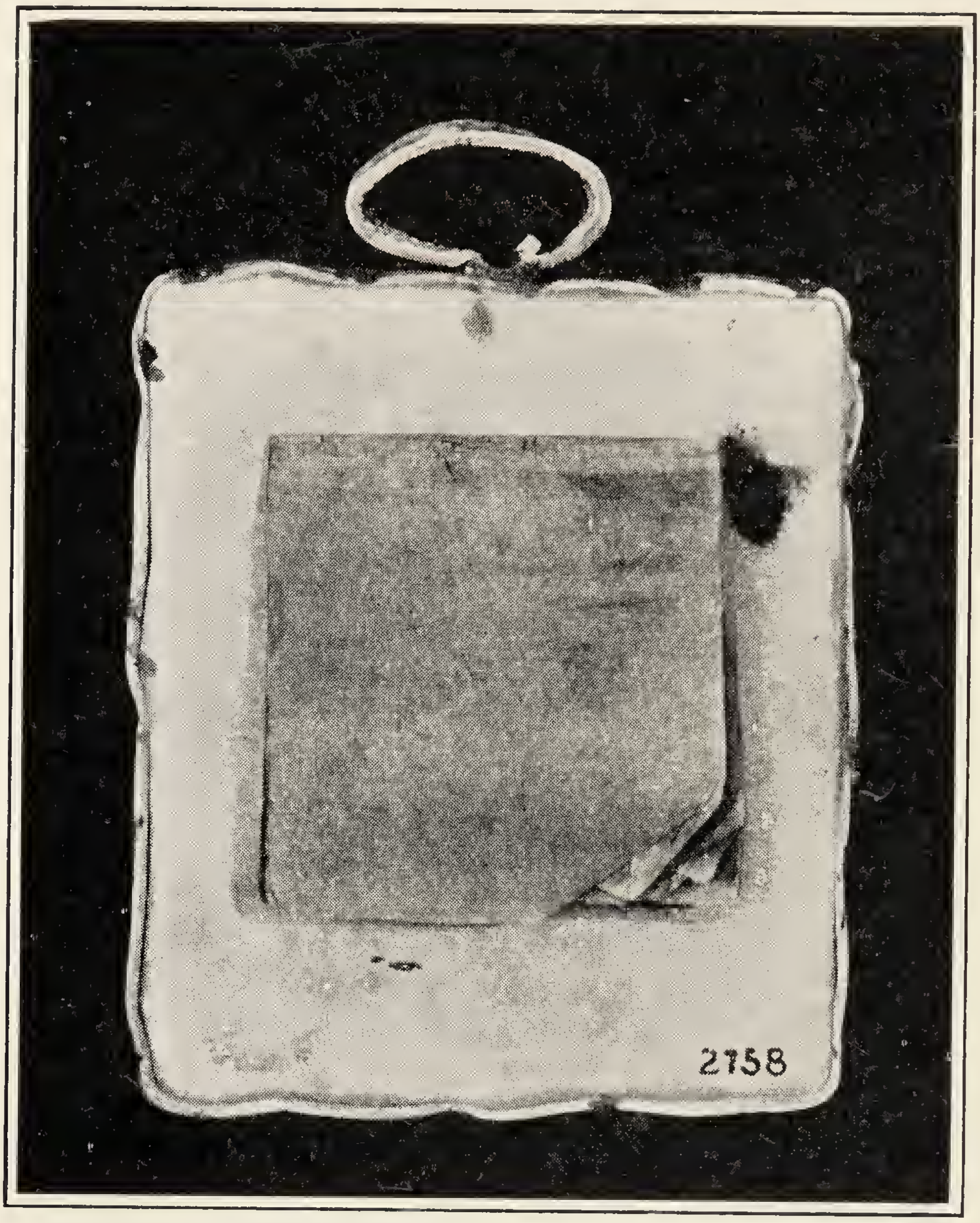


Fig. 3. Relic of Ohm's Apparatus. A leather pad upon which he cut gold-leaf for his electroscopes. A book of gold-leaf.

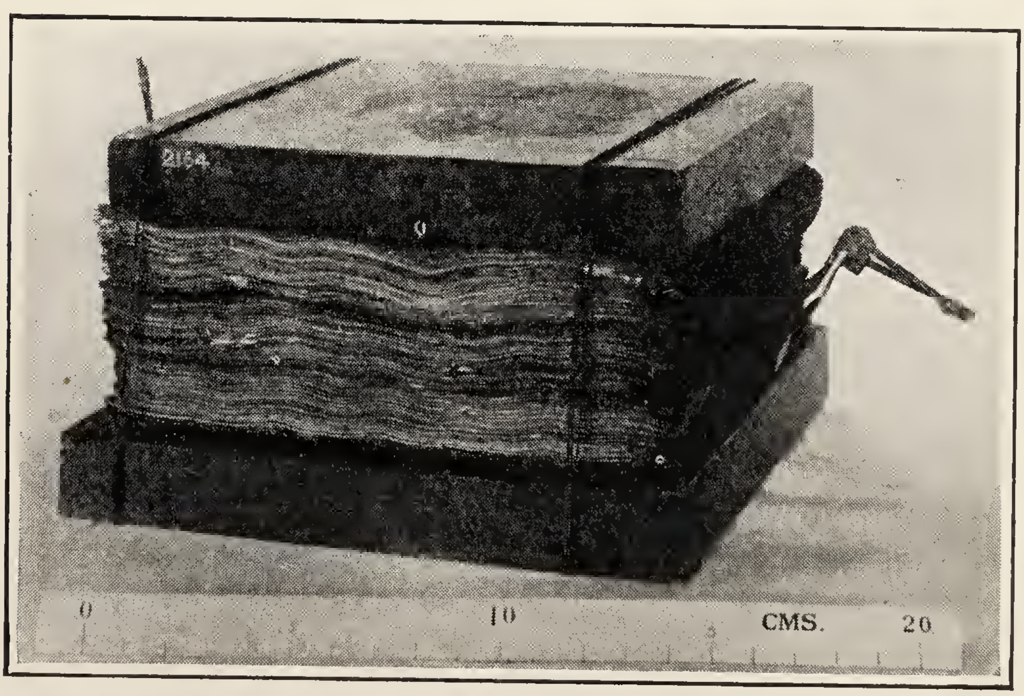


Fig. 4. Relic of Ohm's Apparatus. A dry-pile battery, probably of Zamboni type.

ately, lack of means limited him to but three terms at the University. At the end of September, 1806, by the services of the

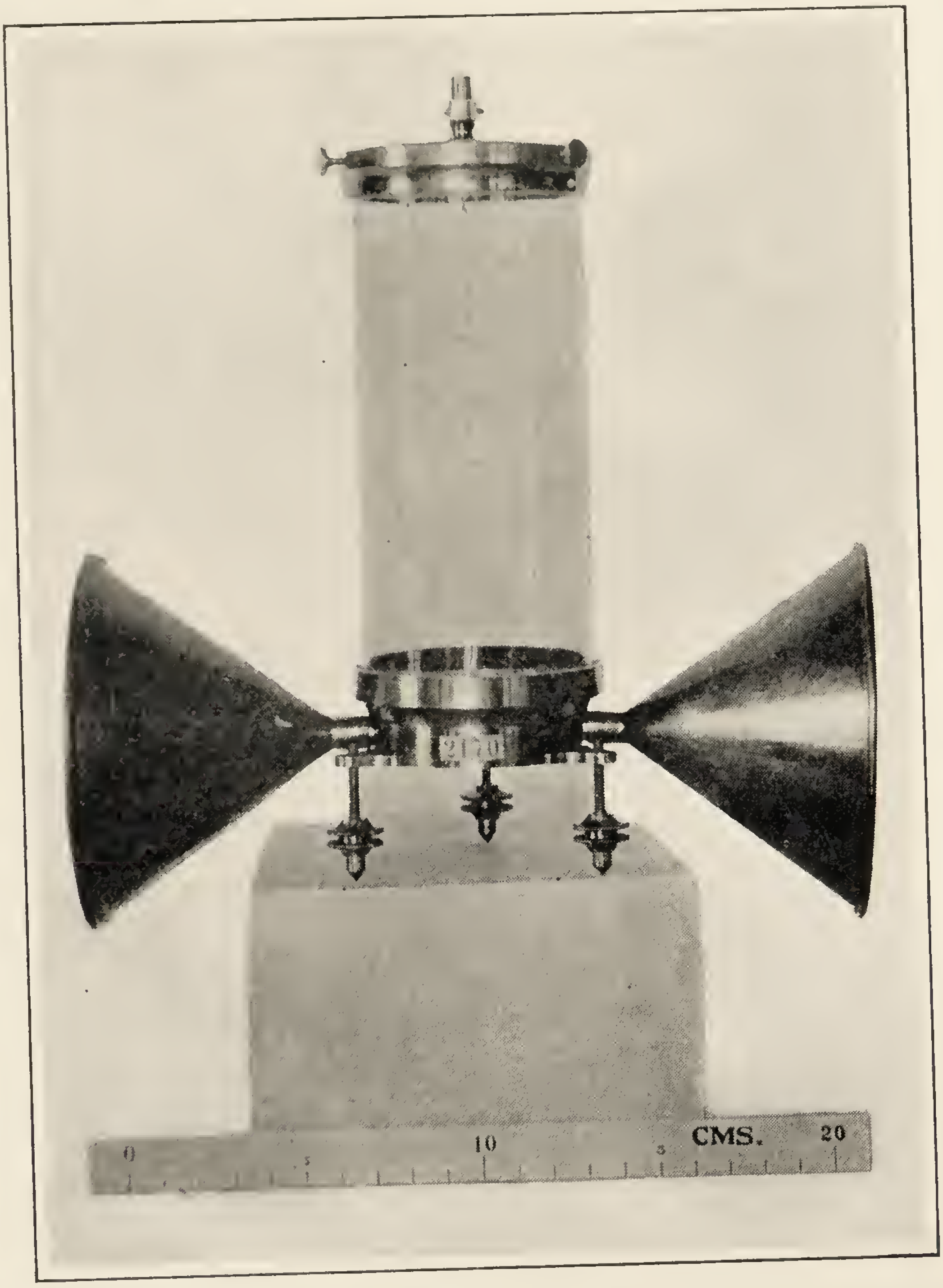


Fig. 5. Thermo-galvanometer used by Ohm. It contains two rectangular bars, probably one of bismuth and the other of antimony, one above the other, at a distance sufficient to allow the lower magnet of an astatic needle to swing between them. The ends of the bars are soldered together, and the upper bar has a longitudinal saw-cut through which the lower suspended magnet can pass to its proper level. The funnel-shaped reflectors are directed towards the solderings.

bookseller Walther, he obtained a post as a mathematical tutor at a school kept by Zehender, a clergyman in Gottstadt, Switzerland. Soon afterwards, the master of the school wrote to Walther:

Ich habe beim ersten Anblick des achtzehnjährigen kleinen und schmächtigen Junglings nicht glauben können, dass dieser der

Fig. 6. Relics of Ohm's Apparatus. Bobbins wound with wire.

empfohlene Lehrer sei, aber mich bald von dieser Tüchtigkeit und Brauchbarkeit überzeugt.

In other words, at first sight he could not believe that this small and weakly youth of eighteen could be the teacher recom-

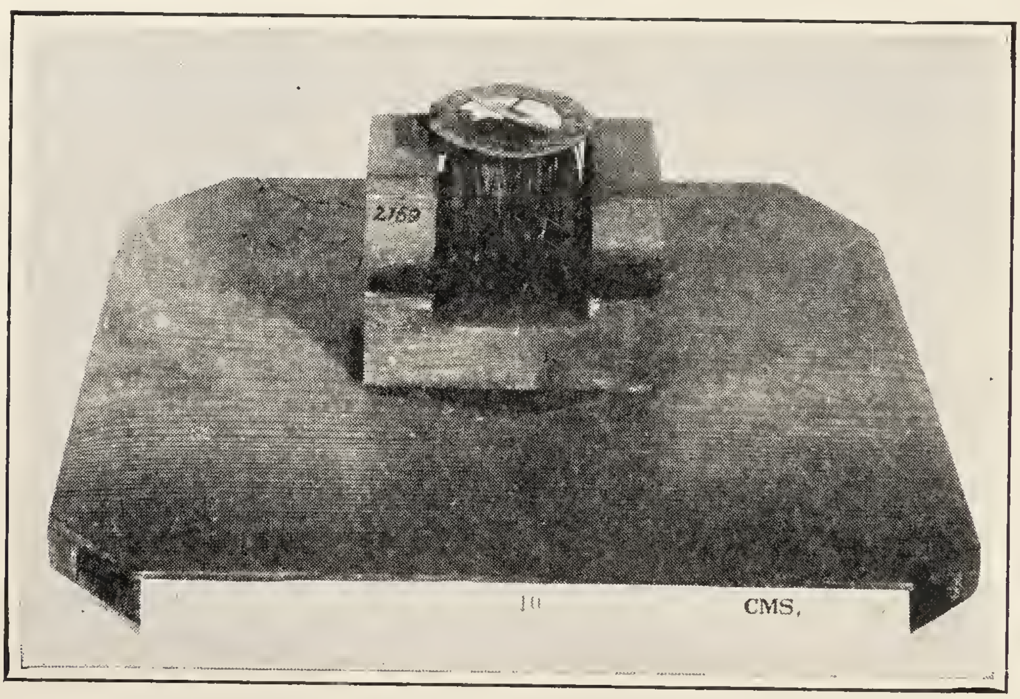


Fig. 7. Relic of Ohm's Apparatus. A primitive galvanometer.

mended, but he soon became convinced of his aptitude and usefulness. After the third half-year at Gottstadt, Georg went to Neuchatel with the object of taking private lessons in

mathematics and conversational French. It was at this period that, upon the advice of his old friend, Professor Langsdorff, he studied the works of Euler and Lacroix. At Easter, 1811, however, he returned to Erlangen and on October 25 of that year he obtained there the degree of Doctor of Philosophy. His

Fig. 8. University Buildings and Hauptstrasse, Erlangen, 1743–1826.

inclination was still towards physics, and his special subjects were now mechanics, light, and particularly colour. For three terms he read mathematics, but for reasons of economy he had again to shoulder what to him was the irksome yoke of a teacher.

He was aware that Professor J. S. Schweigger, of Erlangen, had held an appointment at Bayreuth and had since been

called to the chair of Mathematics and Physics at Nuremberg. In November, 1811, he accordingly wrote to the authorities at Bayreuth to offer his services, but the result was discouraging. He remained at Erlangen, and on July 28, 1812, addressed a letter to the King of Bavaria praying for employment as a teacher. In consequence, on December 16, 1812, Ohm became a tutor at the Realstudienanstalt at Bamberg. There he remained, impoverished and miserable. From the depths of despair he wrote repeatedly to the King and to the authorities, but from the uncongenial conditions there was no escape.

It must be remembered that the year 1813, critical in the history of Europe, brought Germany to the storm-centre of the struggle against Napoleon. Upon Erlangen's 8,000 inhabitants, 33,685 troops were in that year billeted. In addition to the threat from without, there was anxiety lest civil war should arise in Bavaria in favour of Prussian rule. Georg was then twenty-four, of military age, but either upon the grounds of philosophy, physical unfitness, or natural reluctance, he stood aside from military service.

In the spring of 1817, he published his first book. He dedicated it to the memory of his father and gave to it the impressive title, *Grundlinien zu einer zweckmässigen Behandlung der Geometrie als höheren Bildungsmittels an vorbereitenden Lehranstalten*, and he sent a copy to the King of Bavaria, beseeching him to grant amelioration of circumstances. The royal reply was as carefully calculated, but more tactful: there was no appointment vacant, but the book had been placed in the library.

Copies of the *Grundlinien* were sent to other reigning monarchs, and amongst them, fortunately, to King Friedrich Wilhelm III. of Prussia, who looked with favour upon the application. Ohm therefore left the land of his birth, Bavaria, and in the autumn of 1817 took up his quarters in Cologne as Oberlehrer in Mathematics and Physics at the Royal Konsistorium. There he found friends and apparatus, a library, and, above all, greater freedom, response, and appreciation. The physical apparatus at the Jesuit Gymnasium of Cologne enabled him to proceed with the investigation of the galvanic

circuit. He applied himself with complete devotion to his duties, and it is pleasant to record that, in addition to his normal remuneration, he received in October 1822 a "gratifikation" of 100 thalers in recognition of his special services.

His influence and his teaching were now, as in his future career, inspiring. Years afterwards, one of his students wrote:

> . . . seine Art und Weise, sein frisches gesundes Wesen steht mir lebendig vor der Seele, und es gehen selten Wochen, nie Monaten vorüber, ohne dass ich an sie denken muss.
>
> [His nature and manner, his fresh healthy disposition, remain vividly impressed upon my soul; seldom do weeks, and never months go by, but I must think of them.]

His zeal never flagged; he directed his students towards the object which his own genius sought. For a long time his choice alternated between mathematics and physics—mathematics that leads through the mysterious to the wonderful, physics without which mathematics can accomplish little. After the manner of the pioneers, he took care that his mind should not drift. He chose a direct object, and for that, with the utmost skill, he steered. His direct object at Cologne, was the elucidation of the galvanic circuit. He there investigated the relative conductivities of metals, the theory of the galvanometer, and by experiment, the law of flow of electricity in conductors. In April 1826, he realized, however, that if he could break away completely from the restraints of teaching, he could establish the truth concerning electrical circuits. So convinced was he of this that he requested the authorities to grant him leave of absence for a whole year, undisturbed. Leave was accorded in a most gracious and generous manner, and he betook himself to his brother's house at Berlin. He was probably actuated also by a second motive, for, notwithstanding his happy surroundings at Cologne, he was conscious that his achievements deserved recognition in the form of an appointment to a professorial chair, and on December 15, 1818 —only a year after his arrival in that city—he had written to the Royal Prussian Konsistorium requesting that his case might be kept before them in this respect. If he could now produce something tangible, both objects would be secured.

The tangible result of his sojourn in Berlin was, in fact, his book, *Die galvanische Kette, mathematisch bearbeitet,* which was published in Berlin in May 1827.

Confirmation of his law of electrical circuits, described in this treatise, came from Fechner of Leipzig, from Pfaff of Erlangen, and from Poggendorff of Berlin. Criticism, however, was levelled against it, gently enough by Kämtz of Halle, but more provokingly by G. F. Pohl in the *Jahrbücher für wissenschaftliche Kritik*, to which Ohm forcibly replied. Scientific strife led to the breaking off of friendly relations, and Ohm, taking into account the true nature of the opposition, relinquished his appointment at Cologne and, during the six years 1827–33 retired into private life.

His desire for freedom to continue his investigations, his annoyance at the delay in obtaining an appropriate appointment, his irritation at being attacked where he ought to have been supported, explain his action at this juncture; but behind it all there was a common cause. His philosophy, that of arriving at the truth by observation and measurement, clashed with what was then being taught at Bamberg, Jena, Heidelberg, and Berlin. Germany was suffering from bureaucracy tempered by despotism, impressed upon the wreckage of an empire that had been restrained from advance by feudal forms and various animosities. Hegel had arisen to teach that human life is of more consequence than its incidents, destiny was again to be the rule, details were nothing, specialism was to be discouraged, the national conscience was to arise and unify the scattered states into an imperial organization, rising above the finite to the infinite. Hegel had been concerned in the establishment of the *Jahrbücher für wissenschaftliche Kritik*, and in 1827 the popularity of Hegel, expressed in poetry, medals, and gifts of silver mugs, was at its zenith. Hartmann, therefore, discloses the truth when he says :

> Die Hegelsche Philosophie beherrschte zu jener Zeit das Feld. Sie wollte die Naturgesetze auf dem bequemen Wege der souveränen Spekulation, nicht auf dem mühevollen Pfade der Messung und Beobachtung ergründen. Das war nun freilich nicht nach Ohms Geschmack; er war aus einem andern Holtz geschnitzt.

[At that time Hegel's philosophy predominated. It sought to prove the laws of nature, not by the irksome means of testing and observation, but by the convenient method of sovereign speculation. This, of course, was not to Ohm's taste; he was carved from other wood.]

Apart from these initial skirmishes, Ohm's law was for some years scarcely noticed, except by a few physicists. In France, between the years 1831 and 1837, Pouillet demonstrated its truth by direct experiment so effectively, and concentrated his mind upon it so intently, that ultimately he almost thought he had discovered it. The triumph of Ohm came in 1841, when the Council of the Royal Society of London awarded him the Copley Medal for his researches into the laws of electric currents, contained in various memoirs in *Schweigger's Journal*, *Poggendorff's Annalen*, and also in *Die galvanische Kette mathematisch bearbeitet*. The Council declared that in these works Ohm had established, for the first time, the laws of the electric circuit—a subject then of vast importance, and previously involved in the greatest uncertainty.

Ohm, the Council pointed out, had demonstrated that the usual vague distinctions between intensity and quantity have no foundation, and that all explanations derived from these distinctions are utterly erroneous. Both theoretically and experimentally, Ohm had proved that the action of a circuit is equal to the sum of the electromotive forces divided by the sum of the resistances, and that whatever might be the nature of the current, whether voltaic or thermo-electric, if this quotient be equal, the effect is the same. To Ohm also the Council assigned credit for providing means to determine with accuracy the values of the separate resistances and electromotive forces in a circuit. They drew attention, moreover, to the extent to which the labours of Ohm had been neglected. Within the five years preceding the bestowal of the medal, however, Gauss, Jacobi, Poggendorff, Henry, and many other eminent philosophers had acknowledged the great value of Ohm's labours and their obligations to him in conducting their own researches. The special subjects noticed by the Council on this occasion were his researches on the conductivity of metals, on the power of

electromagnetic multipliers (galvanometers), and on the nature of unipolar conductors and hydro-electric currents.

In England, those physicists who had most experience in electrical researches bore the strongest testimony to the help they had derived from Ohm's results. It was confessed that if the works of Ohm had been earlier studied, the industry of experimenters would have been better rewarded.

The comment of Eugene Lommel upon this acknowledgement of Ohm's results was:

> So wurde Ohm vom Auslande her die späte Anerkennung zu Theil, die ihm das Vaterland so lange vorenthalten hatte.
>
> [Thus foreign countries accorded to Ohm the recognition the Fatherland had so long withheld.]

Lommel proceeds to state that after the publication of the work on the galvanic circuit, Ohm's attention was directed to molecular physics.

The eulogy bestowed by the Royal Society upon his discoveries, encouraged him to investigate, by the aid of analytical mechanics, the form, magnitude, and mode of operation of atoms. He wished, in fact, to produce a *Principia* for the microcosmos. Fate, however, stood between this desire and its realization. The dismal years 1827–33 were given by him to mathematical instruction at the Military School at Berlin. He tried unsuccessfully to obtain a better appointment there or at Oldenburg; for the remuneration was less than half what he had received at Cologne. Happily, on July 3, 1833, King Ludwig I. of Bavaria, issued a decree that relieved Ohm of these anxieties. He was given a professorship at the Polytechnic School of Nuremberg, and he retained touch with that institution until 1849. In 1835 he was appointed also to the Chair of Higher Mathematics at the University of Erlangen, and at the same time State Inspector of Scientific Education. Ultimately, he became Rektor at Nuremberg. Towards the end of 1849, he was appointed by Maximilian II., Professor of Physics to the University at Munich; the Akademie of Science selected him as Conservator in Mathematical Physics, and in addition, following Steinheil, he was adviser concerning the development of tele-

graphy for the State. These manifold duties prevented him from continuing his researches in molecular physics. His biographers have found some compensation for this loss in the circumstance that in 1852 and 1853 he published at Munich his results, obtained in the summer of 1851, on interference phenomena and polarized light, and that he discovered how to express his conclusion in a simple formula. Unfortunately, these results, in which two plates of crystal in polarized light could be made to produce a series of concentric coloured ellipses, resembled in many respects those arrived at, quite independently, by Langberg of Christiania, and published in the Norwegian *Magazin for Naturvidenskaberne* for 1841.

Notwithstanding his disinclination for teaching, circumstances to the end obliged Ohm to teach, and he taught well. He was an advocate for individual instruction. The usual two-hour "lecture" was broken up by him; about half was at the blackboard, and the remainder was given to working out examples with his students. In this manner he remained in touch with his class. Throughout Germany, his method left its effect. The conditions, however, under which he taught were opposed to rapid progress. For example, students had forms to sit upon, but no desks at which to write. Their mathematical knowledge at entry was so slight that physics to them was at first unintelligible. He realized this, and in 1852 he devoted his precious time to writing out for them with his own hand complete notes, which were lithographed.

Ohm lived simply. He was of marked energy, of middle height, compactly built, sturdy and strong. He was clean shaven, and his friend Mann says that his physiognomy was of the Martin Luther type. His eyes were large and penetrating; his mouth revealed wit, satire, and good humour. The long dark-blue coat he wore was provided with side-pockets which always held a snuff-box. In diction and phrase he excelled; moreover, his voice was full, and far into his life it retained its attractive quality. If a problem was to be solved, he approached it with his students as though he did not yet know what it would reveal. He encouraged them to find the answer, and at last he would ask: Do you understand? Is it clear? After he had ex-

plained it, it was always clear, crystal clear. Bauernfeind has recorded that Ohm was by nature benevolent. The bitterness of his early rebuff did not rankle. He spoke but little, but what he said was of substance. Beyond the College gates, he was known within his own country but slightly.

His scientific writings were brought together and published at Leipzig in 1892—under the editorship of Dr. Eugene Lommel, Professor of Physics at the University of Munich—with the title *Gesammelte Anhandlungen von G. S. Ohm.* It is a book of 855 pages, relating to 23 communications in which can be seen the results of researches extending over thirty years. The subjects dealt with are comparatively few, but the treatment is thorough, and there is evidence line upon line of tenacity of purpose. From the first, there is manifest the determination of Ohm to establish the law of flow of electricity in metallic conductors. This led him naturally to consider the development of such measuring instruments as the multiplier, or galvanometer, which Poggendorff and Schweigger had devised in 1821. It attracted him also to the results obtained by the English physicists, Children and Davy, with regard to the glow of wires heated by electric currents. Ohm further investigated the question whether the law established for metallic conductors was applicable to liquid conductors. In addition, time was occupied in elucidating the experiments of Erman on so-called "unipolar conductors". Then followed his work in acoustics, particularly with reference to combination tones, and lastly his experiments and theory relating to polarized light.

From these contributions of Ohm to natural science may be singled out three of transcendent value: his law of electric flow, his law of combination tones, his philosophy of research in physics. The law of electric flow is based upon experimental results appertaining to a property of matter. It implies that the potential difference between any two fixed points on a given homogeneous conductor, when the flow of electricity between those points is steady, is a direct measure of the current in the conductor, between those points. The ratio of that potential difference to that current, in these circumstances, is a characteristic of the portion of the conductor in question, and is

called the "resistance". So long as Ohm's law applies, "resistance" thus defined is constant for all values of the potential difference between the two points. In his own words:

Die Grösse des Stromes in einer galvanischen Kette ist der Summe aller Spannungen direkt, und der ganzen reducirten Länge der Kette umgekehrt proportional, wobei man sich erinnern muss, dass jetzt unter reducirter Länge die Summe aller Quotienten verstanden wird, die aus den zu homogenen Theilen gehörigen wirklichen Längen und dem Produkte der entsprechenden Leitungsvermögen und Querschnitte gebildet werden.

[The magnitude of the current in a galvanic circuit is directly proportional to the sum of all the electromotive forces, and inversely proportional to the whole of the reduced length of the circuit, and it must be remembered that by reduced length is to be understood the sum of all the quotients which can be formed corresponding to all the actual lengths of the homogeneous parts and the products of the corresponding conductivities and cross-sections.]

The law is most easily demonstrated to hold in the case of homogeneous metallic conductors at constant temperature.

Ohm's law thus defined is applicable to all conducting systems and is free from ambiguity; its usefulness has carried it into the wider field of electrical research and engineering, where its interpretation has occasionally been stretched to the very limits of its validity. From metallic conductors it has been extended to electrolytes, from electrolytes to dielectrics, from dielectrics to electric arcs, and from arcs to thermionic valves. Moreover, by mathematical devices, inductance, capacity, and leakance have all been operated upon to convert them into terms capable of being interpreted as "resistance", so as to bring them under the jurisdiction of a kind of Ohm's law. A clash consequent upon this struggle for latitude occurred in England in the summer of 1896, when there was a fierce and prolonged debate with regard to the existence, or not, of "negative resistance" in the electric arc. The attack was led by Dr. S. P. Thompson, at a meeting of the Physical Society of London, and was hotly responded to by Professor W. E. Ayrton, in a characteristic speech beginning with the words "It is a pity that so much erudition should be marred by three obvious misconceptions. . . ." After some weeks of acid controversy, it

was decided to refer the matter for settlement to Oliver Heaviside who gave judgment as follows:

> I am asked my opinion about negative resistance. This I take to mean simply that if a body formally obeyed Ohm's law, $E = RC$, and Joule's law, $H = RC^2t$, but with R a negative instead of a positive quantity, it would possess negative resistance. The effects produced by the negativity of R (and other quantities) have occupied my attention in certain papers, and are interesting and instructive. But I have no faith whatever in the permanent existence of a body with negative resistance, on account of the general instability. At the same time I am not prepared to deny that a substance might temporarily, and under suitable circumstances, behave as a negative resistance approximately, especially if it is in a state of continuous material change. . . . Whether the arc may be conveniently regarded in this light, is not for me to say. I do not know enough about the arc. I prefer gas for personal use.—*Oliver Heaviside*, July 28, 1896.

Pioneer work in electrical communication, from the middle to the end of the nineteenth century, was in great measure carried on by electricians whose equipment of theory was limited to applications of Ohm's law in the somewhat ambiguous form

$$\text{Current} = C = \frac{E}{R} = \frac{\text{Electromotive force of battery}}{\text{Resistance}}.$$

When they dealt with single metallic conductors, there was not much difficulty. The application of the formula to networks, and to circuits containing electrolytes, especially if so-called "back electromotive forces" happened to be present, was sometimes troublesome to them. The meaning of the difference of potential between two points was clear, because most of the electricians of that time were familiar with static electricity, and with some form of electrometer. They had, however, to grasp the fact that in applying Ohm's law they were concerned not merely with the electromotive force of their battery, but with electrolytic and other effects, usually involving a counter electromotive force E_1, so that the current was given by

$$C = \frac{E - E_1}{R},$$

where R was known or could easily be measured. With the improvement of galvanometers, there followed the ammeter and the voltmeter. Electricians began to think less in terms of resistances, and more in terms of current and fall of potential along conductors. At the next stage of the advance, attention was directed to concise definitions of electrical power and electrical energy. Ohm's law remained the basis, and carried its way at last into the new field of alternating current theory. This had been forbidden ground, for Ohm's law predicated the steady state, and here all that was constant was inconstancy. Nevertheless, a variety of Ohm's law at last sprang into existence for alternating currents, and it flourished exceedingly. Thus Ohm's law was—as Ohm had prophesied in *Schweigger's Journal* of 1826, it would be—in perfect agreement with experiments in all directions, and characterized by simplicity that extends its application to all experience with electric currents—simplicity such as is only found in truth. Or in his own words:

> . . . wie nur in Gefolge der Wahrheit zu erblicken ist, als das reine Gesetz der Natur verkündigt.
>
> [Revealed only in the quest of Truth, as manifested in the pure law of Nature.]

He started from the fact that when two dissimilar metals, or certain other substances, touch one another, they maintain at the point of contact a difference of potential. He recognized that chemical changes in fluid portions of a circuit introduce complexities that occasionally lead to apparent exceptions. These, until interpreted, amount almost to contradictions. He therefore deferred consideration of the parts of circuits that are subject to chemical change, and he dealt first with a circuit of homogeneous material of the same cross-section throughout. For such a circuit he found by experiment that the slope representing potential, co-ordinated against electrical resistance, is a straight line. This line he plotted, and he proceeded in like manner to obtain zig-zag representations of the fall of potential for composite circuits built up of conductors of various lengths, sections, and materials. Then he showed how to calculate the fall between any two given points along such a composite

circuit. He demonstrated that for the steady state, or, in his words, for a circuit—"deren Zustand bleibend ist"—the current is of equal strength at all points along the conducting system, and that a change of current at any one point corresponds to similar change of current throughout. He stated his law, in terms not of "resistance" but of "reduced length". By "reduced length" he meant the length of a wire—of given material, such as standard copper, and of given sectional area—having a resistance equal to the sum of the resistances of the circuit in question.

He pointed out that this law differed essentially from those of Davy, of Becquerel, of Barlow, and of his own early investigations; the discrepancies in those tentative formulae he attributed to the smallness of the range available in the interpolations by which they had been obtained. His argument was next directed to proving that interchange of the parts of a composite line of conductors has no effect upon the total resistance. He proved that for all points along the conductor, provided that the ratio of potential difference to resistance is constant, the current is constant. The trouble in his experiments arose because the resistance of his battery was large and unsteady. He explained why results of greater consistency were obtained with a thermocouple, where the resistance is small. Then he dealt with problems relating to cells in series and in parallel, the effect of putting a galvanometer into the circuit, and general expressions for the resistance of conductors in parallel. He studied also what happens when a conducting body of considerable size is brought up to a circuit, and he showed that the effect is independent of the material of the added body provided that it is a conductor. He completed the task by mathematical investigation of the current in a circuit when the atmosphere exerts some effect (i) without chemical changes, and (ii) with chemical changes. In this part of his work he led the way to the problem of leakage of transmission lines, and its solution by hyperbolic functions in exponential form.

To convert what at first was an empirical rule, into a physical law of the highest order, it was necessary for Ohm to give it the support of theory. He proceeded to develop a theory of

electric flow, following the results of Laplace, of Poisson, and chiefly of Fourier, with regard to the diffusion of heat. During the years 1807–16, Joseph Fourier had communicated several important contributions upon diffusion to the Institut de France. In 1822 there was published his *Théorie analytique de la chaleur*—which Heaviside in 1895 described as the most entertaining mathematical work ever seen. Its appearance was well timed to influence Ohm. Fourier began with the observation that different bodies possess in different degrees the power (i) To contain heat; (ii) To conduct heat through their substance; (iii) To receive or to transmit heat through their surfaces. Corresponding thereto he defined (*a*) The capacity (la capacité de chaleur); (*b*) Specific conductivity (la conducibilité propre); (*c*) Emission conductivity (la conducibilité extérieure).

Fourier had found that when a metal bar is exposed at one end to the constant action of a source of heat, and every point of the bar has attained its highest temperature, the system of fixed temperatures is distributed along the bar in accordance with a logarithmic law. His next step was a statement to the effect that the slope of any curve at a given point measures in geometry the tangent, in dynamics the velocity, in heat the quantity that flows at each point of a body across a given surface, in a small unit of time. He further laid it down that the quantity of heat that one molecule receives from another in a given time is proportional to the difference of temperature of the two molecules, and he derived, for the temperature at any given point along the bar, an equation of the exponential form that in recent years has become familiar in the corresponding problem of electrical transmission, where attenuation is taken into account.

Ohm assumed that the three constants and the mode of handling the differential equations that had been used by Fourier and by Poisson in the heat problem would be directly applicable to electrical conduction. He therefore introduced corresponding coefficients, and he wrote down the differential equation connecting the rate of change of potential with time at any point along an electrical conductor, in terms of the potential itself and of the second differential of potential in respect to

distance. The current was next expressed as the rate of change of potential with distance, multiplied by a coefficient; and he showed that since there can be no heaping up of electricity at a point, such an expression with appropriate coefficients can be applied to composite conductors, or to branched circuits. Further, a branch might be a liquid, provided that there is a good contact at the surface of separation. His first differential equation was subsequently made to represent the steady state by omitting the time term; the integration was thus simplified, and he obtained an expression for the potential at any given point along the conductor—the law of electrical diffusion, where the diffusion occupies the entire conductor. He considered also what happens when such a conductor is left to itself, and further the effect of bringing into contact with it, at a given point, a mass of metal or other conducting substance.

Maxwell (Art. 333, *Electricity and Magnetism*) paid full tribute to the excellence of this work of Ohm, but he held that Ohm, misled by the analogy between electricity and heat, entertained the erroneous opinion that a body when raised to a high potential becomes electrified throughout its substance, as if electricity were compressed into it. Maxwell pointed out that although this opinion itself was wrong, it led Ohm to employ the Fourier equations to express the true laws of conduction of electricity through a long wire. In these circumstances it is desirable here to record the original statement of Ohm (p. 145, *Gesammelte Abhandlungen*):

Es ist nämlich durch theoretische Betrachtungen sowohl als auch durch Versuche, welche an dem elektrischen Strome angestellt worden sind, keinem Zweifel mehr unterworfen, dass die bewegte Elektricität in das Innere der Körper dringt, und ihre Menge sich deshalb nach dem körperlichen Raume richtet, während es auf der anderen Seite ebenso ausgemacht ist, dass die ruhende Elektricität an der Oberfläche der Körper sich sammelt und ihre Menge deswegen von der Flächengrösse abhängig ist.

[From theoretical considerations and by experiments with electric currents, there is left no further doubt that electricity in motion penetrates the interior of bodies, and that consequently the quantity depends upon the volume of the bodies. On the other hand, it is found that the static electricity on the surface of the bodies

accumulates and that its quantity on this account depends upon the extent of the surface].

It must be remembered that Ohm's task was to bridge the gulf between static charge and the steady state of electric flow. He had to contend with the fact that whereas a static charge resides only on the surface of a conductor, electricity in motion through a wire, for the steady state, utilizes not merely the surface but the whole cross-section and substance of the wire. Was he then in error in the manner suggested by Maxwell? The essential distinction that Maxwell wished to emphasize was no doubt that to which he directs attention in Art. 244 of *Electricity and Magnetism.* In the electrical case, however powerfully a closed conductor may be charged, there are no signs of electrification within it; and an insulated body within it will, when taken out, exhibit no electrical effects. In the thermal case, if such a conductor is raised to a high temperature, the body within it will, after a considerable time, rise to about the same temperature as the conductor itself, and when it is taken out it will be hot. Conducting bodies can absorb and emit heat, but they can neither absorb nor emit electricity. Hence, in electrical phenomena there is complete absence of anything to correspond to capacity for heat, or in Maxwell's words:

> It is impossible to give a bodily charge of electricity to any substance by forcing an additional quantity of electricity into it.

If Ohm had lived to reply to Maxwell he would have responded—"I never said you could." He should be credited with having used the analogue to Fourier's *capacité de chaleur* with mental reservations.

Heaviside, who otherwise appreciated the work of Ohm, re-echoed the complaint that the assumption that a wire possesses the power of storing up electricity in its substance, like heat, is erroneous. In partial vindication of Ohm, however, he added:

> Why he should have come to the right result by wrong method was simply that, whether electricity is stored up in the substance

of a wire, or goes to the surface and stays there, the equations are of exactly the same form.

In the interpretation of Ohm's law by analogies, Heaviside also warned electricians against supposing that electromotive force has to "overcome" resistance, as though resistance were a force of friction. In the sense in which resistance is spoken of in association with Ohm's law, the mechanical analogue of resistance is more closely that of a coefficient of velocity in the case of a body, on a level surface, sliding steadily under the action of a constant force. The constant force (electromotive force) is represented by the product of the velocity (current) and the coefficient (resistance), there being in this analogy no lifting of the load.

Contemplation of the physical contrasts that manifest themselves in the study of the way in which Ohm's law is established—the distinctions between heat and electricity, mechanical friction and electrical resistance, the residence of static charges of electricity at surfaces and the fact that electrical resistance to steady currents is a function of the whole cross-section, added to the part played by magnetic forces, and the mystery in all these circumstances of how an electric current heats a conductor—thus reveals to us how wide are the gaps yet to be filled between the conventional framework of hypothesis that serves so well as a base for calculation within the present range of electrical knowledge and the real machinery and scheme of operation of conductance.

The work of Ohm upon so-called "unipolar" substances deserves attention. It relates to experiments by Jäger, Becquerel, Fechner, Erman, and others, with metallic plates and condensers, particularly with regard to the effect of earth connections and residual charges, and to a group of partial conductors, containing disturbing electromotive forces. It was concerning such results that Biot said that in no branch of physics were there so many differences of opinion and uncertainties as those relating to *galvanismus*, and that there was (1831) scarcely a physicist whose views did not differ from those of every other upon important principles. Becquerel, Davy, Walker, Ritter, Berzelius, de la Rive, and Nobili, were

all conspicuous in this heterodoxy. Ohm saw that much of the trouble arose from defective instruments. He realized that the galvanometer as then constructed did not answer the "Wo und Wie" as he called it. Thereupon he directed attention to the merits of the electrometer that deals with only single points of a circuit, and he advocated the simultaneous use of both instruments, particularly in circuits containing metals and liquids.

Attention became more or less focussed upon an experiment by Erman. He claimed to have discovered a class of imperfect conductors capable of transmitting more easily one of the electricities than the other. For example, into a piece of soap, alkaline and very dry, he introduced two metallic wires each communicating, respectively, with the poles of a battery. The two poles retained their potentials. But, on touching the soap with a conducting body, the negative pole became discharged, and the positive pole acquired the potential it possessed when the soap was removed and the negative pole was put to earth. He found the same result with the dried white of an egg, with the flame of phosphorus, the flame of alcohol, and with other flames, except that it was the positive pole that became discharged in flames. For this reason he introduced the terms "unipolar-negative" and "unipolar-positive".

Ohm interpreted the results as a property not of the substance interposed between the poles, but of the current that traverses the substance, *i.e.* as an electrolytic effect. The soap, in his opinion, was decomposed into an acid and into an alkali. The acid he thought to be of an insulating character, and the acid, he supposed, enveloped the positive wire, preventing the positive charge from passing to earth.

The literature of this and allied subjects throughout Europe, for various reasons, covers an extended period. Ohm's book *Die galvanische Kette* was translated into English in 1841, and into Italian in 1847; Pouillet supplied a translation of part of it in 1837, but it was not available in complete form in French until 1860, when J. M. Gaugain produced an edition with appreciative critical notes. By 1860 it was widely known that Ohm had discovered what was generally called the law of

length, section, and derived circuits, but it was not then generally recognized that this law was associated with a theory that embraced innumerable questions relating to the propagation of electricity.

In 1838 Faraday (*Experimental Researches*, vol. 1, No. 1635) discussed unipolar bodies. He remarked:

> If a unipolar body could exist, *i.e.* one that could conduct the one electricity and not the other, what very new characters we should have a right to expect in the currents of single electricities passing through them, and how greatly they ought to differ, not only from the common current which is supposed to have both electricities travelling in opposite directions in equal amounts at the same time, but from each other! The facts which are excellent, have, however, gradually been more correctly explained by Becquerel, Andrews, and others, and I understand that Professor Ohm has perfected the work in his close examination of all the phenomena; and after showing that similar phenomena can take place with good conductors, proves that with soap, etc., many of the effects are the mere consequences of the bodies evolved by electrolytic action.

Ohm's researches on this question had appeared in Schweigger's *Jahrbuch der Chimie*, vol. viii., 1830, and it is in relation to this that Faraday added:

> Not understanding German, it is with extreme regret I confess I have not access, and cannot do justice, to the many most valuable papers in experimental electricity published in that language.

Ohm's contributions to acoustics appeared in *Poggendorff's Annalen* in the years 1839–43. His researches led to the establishment of his law governing combination tones—by which the human ear proceeds in its analysis.

He showed that the ear can derive the sensation of tone only from that particular motion of the air in which the particles oscillate like a pendulum. Helmholtz summarized the results by stating that every motion of the air that corresponds to a composite assemblage of musical tones is, according to Ohm's acoustic law, capable of being analysed into a sum of simple pendular vibrations, and to each such single simple vibration

corresponds a simple tone, sensible to the ear, and having a pitch determined by the periodic time of the corresponding motion of the air. This law rescued acoustics from confusion. Its effect in the development of innumerable applications of physical science is everywhere to be observed. Yet it remained in comparative oblivion for eight years after the death of Ohm, *i.e.* until Helmholtz in *Die Lehre von den Tonempfindungen* used it to explain the relation of overtones to music. This achievement of Ohm is enhanced by the fact that he possessed no ear for music. Urged by a strong desire to solve the riddle of musical tone, he enlisted the services of a musical friend to supply an ear, and he found this friend in Dr. Kellermann.

Ohm's work was based upon the principle that the truth of what is demonstrated by experiment cannot be denied, that what is based upon hypothesis must only be accepted in so far as it is confirmed by observation, that until theory can be exhibited as precise calculations it is imperfect and does not inspire confidence, and that such calculations are the touchstone of hypothesis. His precept was that where these tests are lacking it is best to defer judgment until better data are available. He studied especially conductivity because he imagined that it would lead to knowledge of the internal structure of matter. Bauernfeind states that Ohm's view was that the attractions and repulsions of magnets should be explained not by supposing the existence of positive and negative magnetism, but by imagining in the atoms (Körperatome) the existence of constant positive and negative currents.

At 10 P.M. on July 6, 1854, on which day, notwithstanding his bodily weakness, he had delivered his lecture, Ohm died. On the following Sunday he was buried at Munich. The grave is in the Sudliche Friedhof which can be reached from the city by way of Sundlingertor Platz and Thalkirchner Strasse. It is near the edge of a path that skirts the front of a building known as the Akadien, within the cemetery. Following that path, the grave is about twenty-three paces from the eastern extremity of the building. It is overgrown with ivy, but the simple stone stands clear, and is inscribed:

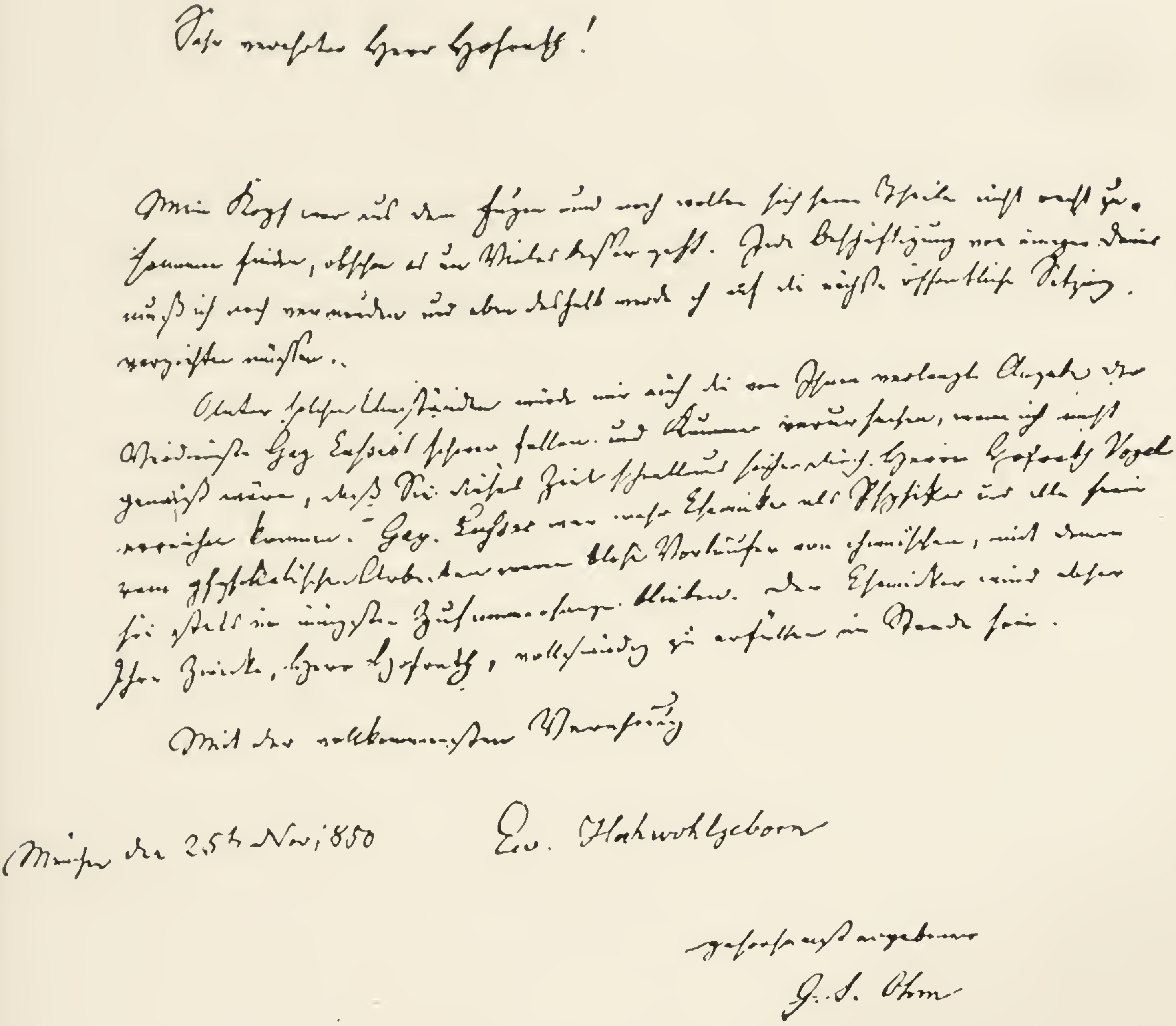
Hier ruht GEORG SIMON OHM KGL. PROFESSOR an der Universität in München Geb. 16 Marz 1787 Gest. 7 Juli 1854

[Here lies
Georg Simon Ohm
Regius Professor
at the University
of Munich
Born 16th March, 1787
Died 7th July, 1854.]

It is unfortunate that a philosopher who devoted his life to precision should have inaccuracy stamped upon what is alleged to be his house, and an erroneous date engraved upon his tombstone. He was born not in 1787 but in 1789.

In 1881, the Electrical Congress at Paris adopted the name of Ohm for the practical unit of electrical resistance, 1 Ohm $=10^9$ C.G.S. units. The standard Ohm is now determined with an accuracy of about 1 part in 100,000.

As an example of his handwriting, the letter (Fig. 9) reproduced from Ludwig Hartmann's book may be examined. It relates to a request made to Ohm by Thiersch for an obituary notice of Gay-Lussac, who died on May 9, 1850.


Sehr verehrter Herr Hofrath!

Mit der vollkommensten Verehrung

München den 25t Nov. 1850 Ew. Hochwohlgeboren

gehorsamst ergebener
G. S. Ohm

Fig. 9. Handwriting of G. S. Ohm.

TRANSLATION.

To Councillor von Thiersch—

My head was in a whirl and is still not quite clear, although it feels much better. I must avoid all continuous occupation, and for this reason I shall not be able to attend the next public conference.

Under these conditions, the information you requested, relating to the merits of Gay-Lussac would be difficult for me to give—this would cause me distress if I were not certain that you could obtain what is required, rapidly and surely through Councillor Vogel.

Gay-Lussac was more of a chemist than a physicist, and all his purely physical work was merely a forerunner of the chemical, to which it always remained most closely related. A chemist will therefore be in a position to fulfil your request completely.

Yours,

G. S. Ohm.

Munich, 25*th November*, 1850.

Oliver Heaviside

VIII

OLIVER HEAVISIDE

Oliver Heaviside, whose name a generation ago was synonymous with all that in the theory of telegraphy and telephony is most abstruse, is to-day acknowledged one of the most brilliant of the English pioneers of electrical communication. His discoveries and teachings provided the impetus that was needed to lift practice from the rut of contentedness with moderate achievement, and to render possible the establishment of a speech and signalling network to embrace the world. During a half-century of intense activity he wrote much. He survived to see his contributions collected, reprinted, and republished in volumes easily accessible. Those books will, for all time, relieve biographers from the task of recounting in detail the scientific and technical battles he fought and won. There is consequently freedom to dwell upon the story of his life, to indicate his relationship with his contemporaries, to glance at his correspondence, to penetrate into his character, to find if possible in what manner he acquired knowledge of mathematics and physics, and to account in some measure for his eccentricities.

His collected *Electrical Papers*, in two volumes, and his *Electromagnetic Theory*, in three volumes, form the chief portion of his contributions to mathematical physics in the particular territory where he ruled supreme. What is available in addition now includes certain of his manuscripts, mathematical notes, and printed books, and a precious selection of his letters, recently acquired by the Library of the Institution of Electrical Engineers. Some of the notes were entered by him with scrupulous care in a series of manuscript books that must have occu-

pied several years in preparation. Beyond the fact that the manuscript books bear marks that indicate that they were purchased in London, there is little to indicate where this mathematical work was done. At or near the time of his death, about fifty of his printed books by various authors, other than the books acquired by the Institution of Electrical Engineers, were sold and scattered. Most of those that were scattered crossed the Atlantic and have there been collected.

Oliver's great-grandfather was George Heaviside, a northern English farmer, who married Elizabeth Winlow, of Dunston near Newcastle-on-Tyne. They had four sons, of whom Thomas, the fourth, was born on July 4, 1785, and died on April 1, 1859, aged seventy-four years. He was a builder and contractor at Stockton-on-Tees. He married Hannah Smith on January 22, 1809, who was born on December 26, 1787, and who died on January 27, 1852, aged sixty-five. They had thirteen children. Thomas, the father of Oliver, was the fourth. This Thomas Heaviside—who was born on October 6, 1813, and who died on November 16, 1896, *i.e.* aged eighty-three—was a wood-engraver, draughtsman, and painter in water colour. He married, on April 2, 1842, Rachael Elizabeth West, born December 17, 1818, died October 31, 1894, aged seventy-six. She was the third child of John Hook West, of Taunton, Somerset, and of Hannah Bowditch, of that town. They also had four sons: Herbert Thomas (born December 31, 1842), Arthur West (born June 30, 1844), Charles (born November 13, 1846), and Oliver (born May 18, 1850). Oliver never married. Charles married; and the sister of his wife was Miss Way, of Homefield, Lower Warberry Road, Torquay, a good soul who for some years extended kindness to Oliver. Homefield was structurally altered in 1927–28 and its name was then changed to Highwold. The Way family was related to the family of Bidder, the "calculating boy". An uncle of Oliver was Sir Charles Wheatstone (born 1802, died in Paris, October 19, 1875), who married on February 12, 1847, Emma West, the sister of Oliver's mother.

The facilities offered to printers by processes of photographic reproduction wrought havoc amongst wood-engravers, and it has been suggested that Thomas Heaviside, the father of

Oliver, migrated to London to seek more remunerative means of livelihood.

To obtain particulars of the birthplace of Oliver, it has been necessary to search at the General Register Office, Somerset House, London, and also to examine the archives at the Town Hall, St. Pancras, London. The result is to discover that he was born at 55 King Street, Camden Town (Fig. 1), and to confirm the date, May 18, 1850. The investigation has revealed that his

FIG. 1. THE HOUSE, 55 KING STREET, CAMDEN TOWN, LONDON, where Oliver Heaviside was born, and where he passed his boyhood. It is the one with the portico.

father and mother became tenants there a few months after September, 1848, probably in 1849. They left this house at some time between 1862 and 1871. The rateable value of the house in 1850 was £35 a year. There is no doubt about the identity, for since the date of Oliver's birth the numbering of the houses in that part of King Street has remained unaltered. That the work of Thomas was of a high order can be inferred from Fig. 2, which is reproduced from an engraving by him of a drawing by Godwin. The inscription written by Oliver in the margin of the proof of this engraving throws light upon conditions at the time.

This was done in less than a fortnight (12 days, I think) under great pressure from Mr. Godwin when father . . . was very ill. £20. 1870. Franco-German war. (He asked £22.) Partly stress. Partly

Fig. 2. Engraving of the Royal Mausoleum, Frogmore, Windsor, by Thomas Heaviside (Oliver's father).

usual practice. Copy in bound volume also. Mr. Godwin had sole permission from Her Majesty to give a picture. Owing to these circumstances, was very proud of it, but it would have been better to have had time to finish it finer. O.H. But note in his book says fell

ill October 15, whereas block is dated June 28. . . . There are four blocks down from October 8 to November 29 and Edwin was on all except first one.

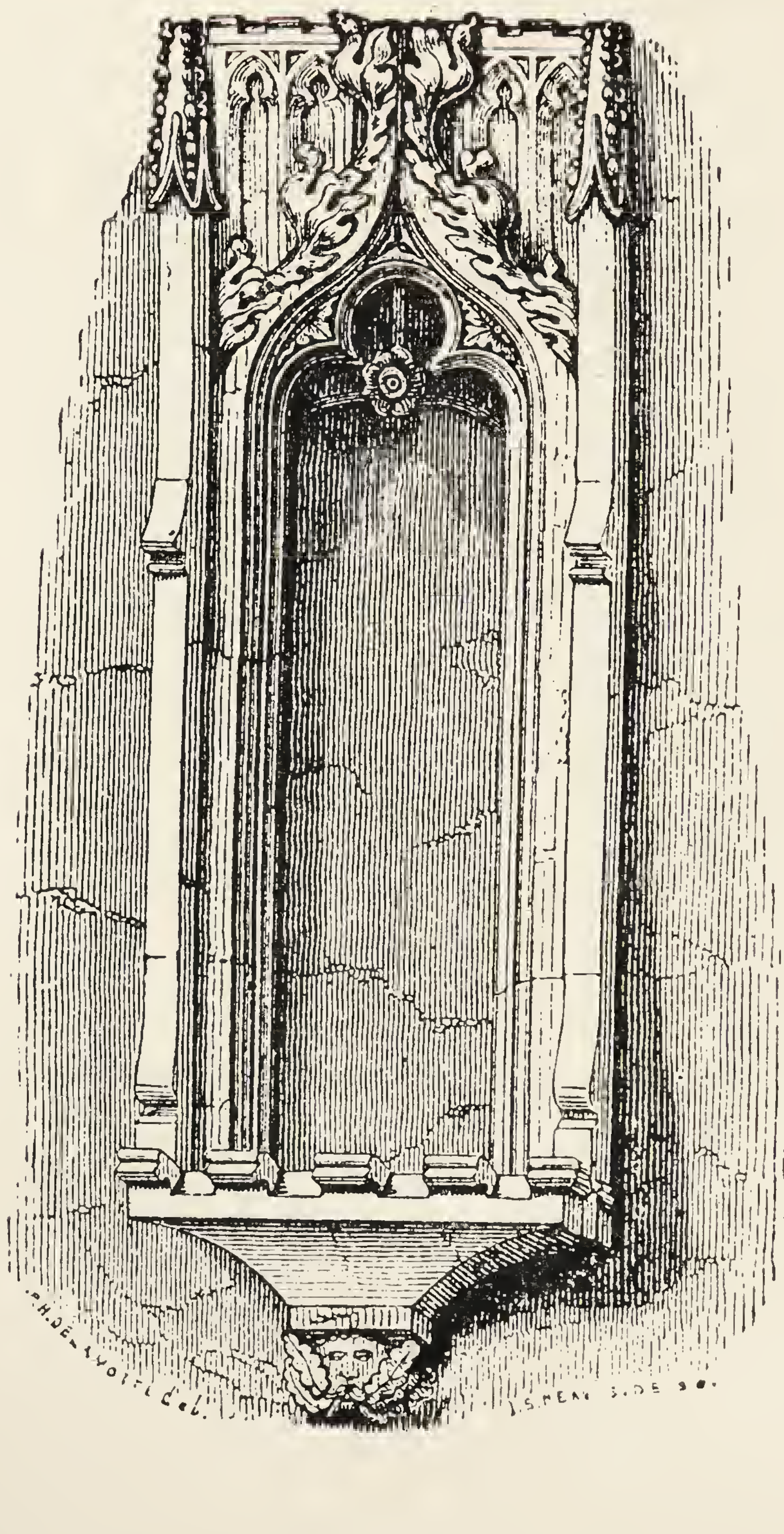
The Edwin referred to was Edwin Heaviside, a wood-engraver, another brother of Thomas. In 1866, Edwin lived at 27 Fetter Lane, London.

An artist and engraver still more distinguished than Thomas or Edwin was their brother, John Smith Heaviside (sometimes Heavyside), whose name appears in *Bryan's Dictionary* and in *Bénézit's Dictionnaire*, as a wood-engraver, in London and Oxford. He was born at Stockton-on-Tees on December 2, 1811, and he died at Kentish Town on October 3, 1864. A small portfolio, undated, now in the Library of the Institution of Electrical Engineers, has pencilled within it "J. S. Heaviside, 6 Belle Vue Cottages, Camden Street, Camden Town". His engravings enrich the writings on architecture of John Henry Parker. An example of his work is reproduced in Fig. 3. The conclusion is inevitable that it was amidst the struggles and victories of art rather than those of science that the boyhood of Oliver was passed. There exist two of his sketches (Figs. 4 and 5) that suggest that he had some early training and considerable skill in drawing. He has himself written under the first: "The Cart Horse. By Oliver Heaviside. Aged 11", and under the second: "2nd work by O. H. (No others preserved)".

Positive evidence concerning Oliver's education is entirely lacking; there is a legend that he was at an early stage taught by his mother. In 1876 his father became the tenant of 3 Saint Augustine's Road, Camden Town, London. Fig. 6. This house fortunately remains, and it has not been renumbered since 1866. It was owned by the Midland Railway Company. To make provision for widening, the neighbouring house was, some years ago, demolished. The rateable value of No. 3 in April, 1889, was £45 a year.

It is certain that from 1876 Oliver subjected himself to strenuous training. His habit was to retire to his room at about 10 o'clock at night and to work there until the early hours of the morning. He closed his door and windows, lighted his oil lamp, and allowed the air to become hot and stifling. He worked

also during the day, in seclusion. In order that he might not be disturbed, his food was placed outside his door, and there it



Kidlington, Oxfordshire, c.1450.

Fig. 3. Kidlington, Oxfordshire, c. 1450. Example of Engraving Work by Oliver's Uncle, John Smith Heaviside.

remained until he was disposed to take it. Thus he transgressed most of the rules that modern conventions prescribe for health.

On the other hand, he enjoyed walking, and occasionally he had more vigorous exercise, for he was a good gymnast.

Amongst his loose papers is a list of gymnastic exercises, and there is also a list, prepared by himself, of measurements that related to him. The figures are here recorded. They are obviously to be interpreted as inches. The height is in agreement with what is remembered of him by a survivor of the family:

(1878.) Height $64\frac{1}{2}$, leg 33, hips $34\frac{1}{2}$, waist 28, chest 35 to 37 (latter under scapula), shoulder width 17, girth chest and arms 44, neck (smallest) 13, biceps 13, below elbow 11, wrist $6\frac{1}{4}$, hand 7, girth palm 8, foot 10, ankle (above) $7\frac{3}{4}$, calf $15\frac{1}{4}$, knee $12\frac{1}{4}$, thigh (groin) $21\frac{1}{2}$, mid thigh $19\frac{1}{2}$, round both knees $21\frac{1}{2}$, mid thighs $31\frac{1}{2}$, width hips 13, girth head (brows) 22, height of shoulder 54.

Briefly, he was a short, red-headed Englishman of autocratic disposition, and of superb powers of mental penetration and intuition, to which was added relentless scorn of masqueraders—some of whom he named the "scienticulists".

To account for his departures from convention, it is necessary to remember that he suffered from deafness. At rare moments his hearing was perfect, and he would then be the first amongst his youthful companions to detect the sound of a distant voice or of the warbling of a skylark on the wing. Years afterwards, one of his manuscripts refers to similar temporary restoration, where he writes:

Episode in the struggle for life. Got rid of deafness partly. . . . Everything in this life that you want comes too late.

Also, he had little if any sense of smell. These defects had their mental counterparts in his qualities, for he became self-centred and possessed of suspicions that too often settled into more or less fixed prejudices. Happily, though his wit was acid and his satire occasionally ill-directed, he was blest with unfailing humour and a disposition that in the end prevailed upon him to forgive even his worst "enemy"—unfortunately then long dead.

In one of his letters to a friend, he speaks of music as having been one of his recreations:

Fig. 4. Drawing Inscribed "The Cart Horse. By Oliver Heaviside. Aged 11".

Fig. 5. Drawing Inscribed by Himself "2nd Work by Oliver Heaviside (No others preserved)".

. . . In old days I went to concerts, very long and highly classical; I always got wearied. I could not take it in—except the divine Schubert. Now there are a lot of very fine overtures of the Freischütz

FIG. 6. THE HOUSE, 3 SAINT AUGUSTINE'S ROAD, CAMDEN TOWN, where Oliver Heaviside resided from 1876 to 1889.

type. People hear them again and again, and so get to know them. May their performance be never discontinued.

. . . I am very deaf. . . . I have no technical knowledge (of music) nor am I a pianist, though I once taught myself B.'s Opus 90. I liked it better than anything else. Truly the conflict between the intellect and the heart.

In those "old days" he also devised a musical notation in-

tended to be easier to read than the orthodox system of lines, bars, and notes. Later he found some pleasure in playing an "Æolian". The instrument he used still exists.

Only here and there is it possible to obtain a tangible clue to his personal history in his middle life. According to one account, after leaving school, he held a post with the Great Northern Telegraph Company, at Newcastle-on-Tyne. Positive proof of this has not been found amongst his papers. There is, however, a statement made by himself that he first observed a phenomenon relating to signalling through heterogeneous telegraph circuits at Fredericia, so that he evidently visited Denmark. From July, 1872, when he made his entry into technical and scientific literature, the surest guide to his career is his published work. An elder brother, Arthur West Heaviside, was a Divisional Engineer of the British General Post Office at Newcastle-on-Tyne. For a considerable period, Oliver worked closely with him, especially at solving such electrical problems as arose. There is a memorandum to the effect that on January 15, 1873—perhaps the happiest day of his existence—Oliver and Arthur made experiments "on duplex working with an artificial line and rough resistances at Beckett's shop". With single-needle instruments, they met with complete success "obviously to the delight of all parties". They proceeded to arrange a similar system between Newcastle and Sunderland. Messages were sent "simultaneously from both stations as fast as they could be transmitted by key". An entry in his note-book, relating to his paper in the *Philosophical Magazine* for June, 1873, states with satisfaction: "I was credited in America with having described quadruplex first, or suggesting it." His paper that followed it in the same journal in 1874, on the working of cables with condensers, was, he says, "the first that took terminal apparatus into account at all". In 1878 he directed attention to the importance of self-induction in telegraph instruments and telephones. Another glimpse of what he was doing is obtained from a draft agreement, dated September 14, 1880, relating to a patent for neutralizing disturbances in cables. The next year there was a movement, on the part of his brother, to obtain for him other employment. At that time, the Chief of the Engineer-

ing Department of the British Post Office was William Preece—who was afterwards Sir William Preece. A letter to Oliver from Arthur explains what was in contemplation:

(*November* 22, 1881.) Preece states that the Western Union of America are about to adopt the Wheatstone, having ordered 24, and that he has the nomination of about six clerks to manage them, with salaries of about £250, and then he asked after you and I told him you were a student still—obvious—should you apply I believe he would nominate you.

What transpired is not recorded, but there is no doubt that, in June, 1882, this "student" was at 3 Saint Augustine's Road, experimenting at home with microphones, particularly to ascertain the effect of pressure at the contact surfaces. His apparatus consisted of a battery, carbon contact-blocks, a watch, and a galvanometer. His note upon these experiments reveals him still an imp, and his father an experienced observer:

Father smells acid in the room. Two or three evenings. I said, at hazard, it was the electricity. Query, ozone generated by sparking, or nothing to do with it. Father says it is just like the battery he made when he was a boy, and that it is my battery. I didn't say it wasn't. What is the best arrangement to get the greatest variation of resistance in the circuit? I find that the internal and external resistance must be equal.

Among his smaller manuscript notebooks now treasured in the Library of the Institution of Electrical Engineers is one that contains what he describes as "An abridged account of experiments in May, 1886, London". It relates to various arrangements of Wheatstone bridges, and it includes notes on condensers, telephones, galvanometers, relays, batteries, inductance balances, interference tests, lightning, and cohesion. He there refers to an experiment made by him as early as 1868. Another of these notebooks gives an account of induction experiments carried out by him in July, 1883. The part he took in seeking for a "theoretical explanation" of the experiments carried out by his brother, A. W. Heaviside, in Newcastle, is alluded to in paragraph 216 of vol. 1 of *Electromagnetic Theory*, and in *Electrical Papers*, vol. 2, p. 323.

Referring to his paper in the *Philosophical Magazine* of November, 1886, he has an entry:

> Most remarkable fact, was speaking by telephone between two circuits, $\frac{1}{4}$ mile square, $\frac{1}{2}$ mile between centres.

It must always be regretted that circumstances so soon placed him out of direct touch with practical telegraphy and electrical apparatus; for his skill in experimental researches would inevitably have led to important developments. Moreover, in experiment, he would have found the natural antidote to ills consequent upon unremittent concentration upon theory.

At the prime of his life there was a dramatic change in his environment. In the autumn of 1889, Oliver with his father and mother left London and took up their residence at Paignton, in Devonshire. This was partly on account of his parents' failing health, but it was also because his brother Charles was able to offer accommodation; for Charles, who had a music shop in Torquay, had lately opened a branch at Paignton. Oliver's mother died at Paignton on October 31, 1894. His father died there on November 16, 1896. From about 1897 to midsummer of 1909, Oliver dwelt in comparative solitude at a house known as Bradley View, in the neighbouring town of Newton Abbot, near Dartmoor. At Newton Abbot, he suffered tortures on account of molestation by boys of the locality. A more serious trouble was a bad attack of malarial jaundice. Miss Way, though herself old and frail, thereupon offered him quarters at the house, Homefield (Fig. 7) in Torquay, which ultimately became his by purchase, and where he remained to the end of his life.

The portraits of him, reproduced in the Frontispiece, probably were taken in the year 1893. They are from old negatives long stored in a cardboard box bearing the inscription in his own hand:

> NEGATIVES of photographs of the present Dr. Heaviside—(taken by C. T. H.) at Palace Avenue, Paignton. Keep dry and the film sides in contact. The one with hands in pockets is perhaps the best, though his mother would have preferred a smile.

It must have been the portrait "with hands in pockets" that

his friend Professor FitzGerald contrived to obtain from him in 1898, and concerning which FitzGerald wrote:

> I am sorry you did not give a less retiring view of your face. If you were one of those who had a great reputation for getting on by brazen pushiness, I could understand your fearing that your portrait being published might be misconstrued.

Heaviside scoffed at those who publish their photographs:

FIG. 7. HOMEFIELD, TORQUAY, now known as Highwold.

> It makes the public characters think they really are very important people, and that it is therefore a principal part of their biz. to stand upon doorsteps to be photographed.

Concerning a contemporary photograph of a group of members of the Institution of Electrical Engineers he remarked:

> Giants at the back. Pigmies at the front. I gave it, framed and glazed too, away to a Newton Abbot furniture dealer, for nothing, along with an old kitchen table.

In Fig. 8, which is from a photograph of members of the Heaviside family, taken at Berrypomeroy Castle, Totnes, about the year 1893, Oliver is just to be seen in the far background, smoking his pipe. His father and mother are standing side by

side near the centre of the picture; and next to his mother, on her other side, is Miss Way. Arthur West Heaviside is standing on the extreme right of the picture holding his hat in his left hand. Immediately in front of Oliver is Basil Bell Heaviside. Charles Heaviside is stooping down between his father and his brother Arthur. In front of Arthur is Colin, the third son of Arthur. Miss Ethel Heaviside is standing behind, between Oliver's father and mother.

FIG. 8. GROUP OF THE HEAVISIDE FAMILY.

Here, for a moment, it is well to consider his mode of working and his attitude of mind. Throughout his mathematical work in its final form, his writing was singularly neat, although it appears from marginal notes that he was often troubled to find a pen that suited him. An editor, hard to please in other respects, was constrained to confess: "No other contributor can approach the admirable clearness of your copy and the cleanness of your proofs." Mathematical solutions were not obtained by him on all occasions easily—they were frequently the result of strenuous labour and repeated trials. In proof of this, scattered amongst the drafts of his attempted solutions of

problems, his perplexities at moments find expression in interjected remarks:

> This is fearfully complex. . . . Don't see how to simplify it at present, but I think it will work down to something simple in the long run. . . . Which is *right*? . . . Winna du! . . . No go! Save all this bother. . . . Notes on the Brays of the British Asses! Notes on the Brays! (British Association members). . . . Hope. Hope. Hope. Swine. Swine. Swine. . . . All this is very fishy. . . . Revise it. . . . The subject is enshrouded in difficulties and ambiguities. . . . I cannot manage it to-night. . . . Quite fagged. . . . One success only leads to more failures. . . . *Hinc illae lachrymae*. . . . So it is dubious. . . . Patience. . . . Bah!

For preliminary drafts of his mathematical writings he often used the backs of waste invoices from his brother's music shop —an investigation of a Fourier's series thus finds a few square inches on the reverse side of a stray account to somebody for a copy of *Love's Old Sweet Song*, concerning which, alas, no trace of a corresponding term appears in any of Heaviside's equations.

In his extensive numerical work he would have saved himself much trouble if he had used a slide-rule, but he preferred to employ ordinary arithmetic, and occasionally a book of logarithms. He ciphered in bold figures, and so long as there was hope that a solution might be found, he was never thwarted by the labour of the calculations. He possessed a strong desire, amounting almost to a passion, that his work should be published as written. Ultimately, he nearly achieved his purpose. It was a requirement beset with difficulties, many of which were self-imposed. He wrote:

> Experience has taught me that the refusal of a paper by any journal, for unconvincing conventional reasons, implies that the paper is unusually original and good. Fact!

There was, however, another side to this. In 1886 his wrath was kindled against an official who was suspected by him of having hindered the issue of a description of a system of telephony with which his brother had been associated. The charge was that this official "fell foul of it in a savage and even insulting manner and blocked the paper". To make matters worse, the technical

press, at the zenith of his activities, was passing through an exasperating period of litigation. Even his staunch supporter, C. H. W. Biggs, then editor of *The Electrician*—who, before the scientific world had learned to bow to the prerogative of Heaviside's genius, exhibited tolerance towards his obscurities and tact towards his idiosyncrasies, and who at last was thanked by him, "for the opportunity he gave me of exercising my philanthropic inclinations"—was constrained to admonish him:

(*May* 30, 1887.) I would use your letter if I could, but it is dangerous in the present state of the law. . . . I may tell you that at present six of us have two libel suits each against us, or a round dozen altogether, and I venture to think that the cost even if we successfully defend ourselves will be considerable. . . . Candidly then I am afraid to use it, not personally, but . . .

(*June* 1, 1887.) Do you not think it rather infra dig. in a scientist to be so moved at the doings of a scienticulist?

Several years afterwards, Heaviside recorded that in 1887 he came for a time to a dead stop, exactly when making applications in detail of his theory "with novel conclusions of considerable practical significance relating to long-distance telephony in opposition to the views at that time officially advocated". In that year also the editor of the *Philosophical Magazine* found that as no one, so far as he could discover, read Heaviside's articles, he could no longer afford space for them.

In 1888 another editor complained that the articles, so far as could be ascertained, were only read by a few professors, and "Professors, you see, do not advertize". Refined chastisement was also administered on October 31, 1891, from another quarter, when he received a letter as follows:

I am desired to return you the thanks of the Royal Society for your Paper "On the Forces, Stresses, and Fluxes of Energy in the Electromagnetic Field" and to inform you that the Committee of Papers have directed it to be published in the *Philosophical Transactions*. . . .

P.T.O.

Both our referees, while reporting favourably upon what they could understand, complain of the exceeding stiffness of your

paper. One says it is the most difficult he ever tried to read. Do you think you could do anything; viz., illustrations or further explanations to meet this? As it is, I should fear that no one would take advantage of your work.—Yours truly, RAYLEIGH.

With reference to this he records in his note-book:

It took six months before I heard that the paper was accepted for the Transactions. Then 3 months to get it set up in proper type. About 4 months (or 5) more before published. Great delays in correction. Printers humbugs.

Again, Heaviside resented the action of the Royal Society when the Secretary, on July 26, 1894, wrote to him:

I am desired to return you the thanks of the Royal Society for your Paper "On Operators in Physical Mathematics, Part III." and to inform you that the Committee of Papers, not thinking it expedient to publish it at present, have directed your Manuscript to be deposited in the Archives of the Society.

His annoyance in respect to delays of this kind lasted to the end of his life.

The same was true of his books. Vol. i. of *Electromagnetic Theory* took three years to put through; vol. ii. occupied a little more than four years. He calculated that if vol. iii. was to be first presented as serial articles it would consume twelve years. So he gave up the idea of proceeding in that way.

It is necessary here to remember that Heaviside was not a mere equationist. He was a man of high purpose—a reformer. To establish his reforms he had to encounter conventions, and thus he was forced to think, to appear, and to act, as an individualist. In 1913 he wrote to a friend:

Pray don't ever call me a mathematician. I am a physical mathematician or mathematical physicist, and repudiate all mathematicians.

This is the key to a great deal that is puzzling in his writings. In his time, a school had arisen that was intent upon finding physical significance in mathematical processes. With this movement Heaviside was in whole-hearted agreement—but he went further, he made a direct attack upon extremists among pure mathematicians. He said:

Three pernicious results of looking for over much rigour may be mentioned. First, its enfeebling action on the mind . . . secondly, it leads to the omission from mathematical works of the most interesting parts of the subject because the authors cannot furnish rigorous proofs. Thirdly, it leads to mental inability to recognize the good that may be in other men's work should it fail to come up to their standard of rigour.

To account for his abstruseness, there is primarily the fact that his mathematics was of a higher order than could readily be grasped by his contemporaries. Like Newton's *Principia*, his works were for this reason destined in any case to be more admired than studied. He confessed that he was a voice crying in the wilderness—for vectors. His obscureness was further deepened because, following Maxwell, he strove to express his results in the most general form—not always as explicit cases that would have been more readily understood. To add to the difficulties, there was lack of continuity arising from transformation of serial articles and scattered memoirs, irregularly issued, into a treatise. His worst fault, however, was the omission of steps in the argument, especially when the breaks arose from needless digression to attack quaternions, scienticulists, 4π, or an "enemy".

He once expressed his motives thus to an editor:

(*November* 1, 1890.) Posterity. I don't think posterity will care to go to the British Museum to read up back volumes of the . . . I write, or rather wrote, for the present generation. It was a stiff-necked generation. But I am assured by competent judges that I made some impression upon it in spite of its objection to be born again. . . . I never have, and do not intend to have anything to do with *contentious* discussions, as I understand "contentious", contending for the sake of contending, about nothing worth contending about. Legitimate scientific reasoning means that if anyone puts forward views which I consider wrong, and am interested in, and the matter is worth correcting, I have the right to do it, on scientific grounds.

With regard to learning, he said "sit down and work—all that books can do is to show the way". He insisted that we are from first to last in contact with those quantities which are believed to have physical significance—instead of with mathe-

matical functions of an essentially indeterminate nature—and also with the laws connecting them in their simplest form. His advice was to avoid "groping after mare's nests"—for example, in electrical theory, to shun potential—and to keep as close as possible to such variables as are known. He was proud of having been at one time a "practitioner" himself, and his correspondence shows that when practical men approached him in a way of which he approved he was ever ready to assist them, as well as men of science, with their problems.

Repeatedly he was asked by editors to have mercy upon their readers, to write less "poetry" and more "prose", or to write an easy synopsis—but he was remorseless and relentless. His reply was:

> Synopsis? Can't. The Lord will provide. He always does. I am aware and so is everybody that the practicians only glance at my articles, and that the readers thereof are a small minority. It always was so, save a few exceptional articles, and it always will be so. I am afraid you will think the above very unsatisfactory in relation to commerce. I can't help that, though I am very sorry. . . .

Let us glance at the range of subjects at which this reformer worked. Physical science, when Heaviside entered it, had for two centuries been the battle-ground of contentions about the ether, vortex motion, light, electricity, magnetism, and gravity. During his boyhood and youth, Faraday, Thomson, Tait, Stokes, Rayleigh, Helmholtz, Weber, Gauss, and Maxwell had swept over the field. Moreover, an Atlantic cable had been made and laid, land telegraphs in a multiplicity of forms had been successfully developed, and practical details concerning the behaviour of dielectrics and transmission were thereby accumulated far beyond the range of what was then contemporary theory.

Advance in electrical communication systems in his time called chiefly for improvements in methods of measurement of the quantities to be dealt with in practical telegraphy and telephony, and for the interpretation of results in quantitative terms. To prepare himself for the tasks thus suggested, Heaviside faced the drudgery and self-sacrifice needed to render himself efficient; he grasped firmly the weapons of mathematics and

practical experience, and he strode into action at the point where the work was most strenuous.

The article—his first—in the *English Mechanic* of July 5, 1872, is concise and convincing. Of like quality are his papers in the *Philosophical Magazine* of February, 1873, on the best arrangement of Wheatstone's bridge for measuring a given resistance with a given galvanometer and battery, and on duplex telegraphy. The years 1874 and 1876 are characterized by marked development; his papers on telegraphic signalling with condensers (*Phil. Mag.*, June, 1874), and on the "extra" current (*Phil. Mag.*, August, 1876), show that, equipped with the calculus, this youth of Camden Town had made Fourier's theorem his own, he had mastered what Sir William Thomson had revealed with regard to arrival-curves, and he had gone a long way towards interpreting Maxwell. Thus he was able to attack some of the new and then incomprehensible problems that accompanied the introduction of the Bell telephone. To the mystery of such questions he refers in his analytical paper—a treatise in itself—on electromagnets (*Jour. Soc. Tel. Eng.*, 1878), where he remarks that "this most sensational application of electricity appears to be very indifferent to resistance (sometimes), it being said to be sufficient merely to make earth 'through the boot and a blade of grass' ".

Telephonic currents were thenceforward to be his principal theme. If at this stage the telephone had not won his attention, he would probably have devoted himself to designing dynamos; for his paper, *Jour. Soc. Tel. Engs.*, June, 1881, on magneto-electric current generators was full of promise. His two great contributions, however (1) On induction between parallel wires, and (2) On the theory of the propagation of current in wires, written respectively in 1881 and 1882, gave direction to his efforts, and firmly established him as a leader in rational telephonic and telegraphic engineering; for it was Heaviside who finally deposed guesswork, and who provided means that ultimately led to precision in telegraphic and telephonic transmission.

It was in the autumn of 1882 that he began his famous "Electrician" series of papers, about 500 pages, on electrical

theory—vectors, potentials, units, energy, thermo-electricity, propagation of electrical disturbances through a medium, induction balances, models of viscous liquid—all mental equipment for the age of telephony and power transmission that was about to begin. This series led to contributions in 1885 that continued to the autumn of 1887, on electromagnetic induction and its propagation. To him, the ether was "the great storehouse of energy". He studied the mechanics of a rotational ether in which magnetic force is allied with rotation. Maxwellian as he was, he complained that Maxwell had left gravitation out in the cold, and he directed attention to the fact that electrification is always found associated with matter. Although, to gain the advantage of symmetry—and to be able to treat electromagnetic problems as elastic solid problems—he established, as nearly as he could, parallelism between electricity and magnetism, he laid emphasis upon the absence of a magnetic conduction current. Thus magnetism to him, as to all the pioneers, was the transcending mystery of the universe. He introduced, in 1885, for those purposes of symmetry, the fictitious quality: "magnetic conductivity". To describe the general parallelism, he adopted, not quite happily, the word "duplex", and in this sense he exhibited the electric, magnetic, and electromagnetic equations in "duplex" form; *i.e.* symmetrical with respect to electric and magnetic notions. The immediate objects of his attention then became the corresponding *fluxes* and their variations. Potentials he relegated to a secondary place. So far as they concerned the state of the medium, potentials were in fact treated by him as auxiliary quantities, devoid of physical significance.

The experiments of Hughes, in January, 1886, confirmed Heaviside's theories of surface conduction along wires. In the spring of that same year, Heinrich Hertz definitely established by experiment the wave character of electromagnetic transmission through space and through wires. The experiments and writings of Hertz produced upon Heaviside a profound and stimulating effect.

The introduction to Heaviside's papers on self-induction in wires, beginning with his article in the *Philosophical Magazine*,

1886–87, indicates that he was led from his early experiments on Wheatstone's bridge to his investigation of the induction and resistance of long solenoids containing cores, and thence to the mathematical study of the transmission of current "into" wires by diffusion from the external dielectric. With these papers may be grouped his remarkable article, written in 1887, but not published until 1892, on telegraph circuits, and his communications to the *Philosophical Magazine* of December, 1887, on resistance and conductance operators, the whole comprising, in about 200 pages, the foundation of modern theory of telephonic and telegraphic transmission, united with dynamics in the conception of forces and stresses. In effect, Heaviside's Expansion Theorem enables an *explicit expression* for the currents as functions of the time to be derived, for any network, from the *differential equations*, by means of intermediate *operational equations*, under the conditions (i) That the currents are initially zero, and (ii) That given potential differences are applied at various given points of the network. Or conversely, his theorem enables the potential differences to be derived from the currents. His method consists in prescribing rules for obtaining the operational equations, and rules for translating the solution of the operational equations into solutions of the original differential equations.

Another group of investigations, almost inseparable from the preceding, had its origin in his article in the *Philosophical Magazine* for February, 1888, on electromagnetic waves, especially in relation to the vorticity of the impressed forces, and on the vibrations of electromagnetic systems. By "vorticity" he meant the "curl", familiar enough at that time to a few Cambridge, London, Dublin, and Scottish mathematicians, but to the rank and file of his contemporaries, a sore perplexity. Lastly came his treatment of electromagnetic radiation, and the Heaviside Layer, concerning which it may be helpful to observe that in vol. iii. chap. x. of *Electromagnetic Theory*, 1922 edition, p. 331, his article bears the superscription:

> Theory of Electric Telegraphy. *Encyclopaedia Britannica*. Tenth Edition. Reprinted by permission of the proprietors of *The Times*. Written June 1902.

This forms part of his treatment of "Waves in the Ether". He begins with the case of radiation between two coaxial conical conductors with a common apex. The radiation is supposed to go from the inner to the outer cone symmetrically in spherical sheets, with the apex as centre. By assigning various values to the semi-vertical angle of the cone or cones, he derived several cases, some corresponding to "wireless" transmission—a flat plane and a vertical wire projecting from it at the apex, spherical waves from the apex, waves from a Hertzian vibrator, two parallel wires, waves along wires containing sharp bends, and "wireless" waves across sea-water. He explained that the waves accommodate themselves to the surface of the sea in the same way that waves follow wires. Then comes the important statement:

> The irregularities make confusion, no doubt, but the main waves are pulled round by the curvature of the earth, and do not jump off. There is another consideration. There may possibly be a sufficiently conducting layer in the upper air. If so, the waves will, so to speak, catch on to it more or less. Then the guidance will be by the sea on one side and the upper layer on the other.

There is reason to suppose that this article was published towards the end of the year 1902, and that it was the last the editor of the *Encyclopaedia Britannica* (tenth edition) received before publication of the volume in which it is contained. Priority for the layer is not discussed by Heaviside. Regarding priority, therefore, the wise will follow the advice of Newton touching the origin of the theory of light: "To avoid dispute, let every man here take his fancy."

Amongst Heaviside's loose manuscripts is a draft in which he has recorded that in 1902 the cable companies were already considering what the effect of wireless telegraphy might be upon their enterprises. He entertained doubt whether it would have serious influence upon them unless a quick method of automatic wireless signalling were adopted. He was at that time prepared to be consulted as a scientific man, and as an old "telegrapher", but "he was not disposed to do sums set by unscientific practicians who despised mathematics". He saw that what was needed was experiment. To his mind, the scien-

tific work had been done. He had shown the way to improve cable telegraphy—beyond what was possible by more copper and less capacity—"first by the principle of increase of inductance, and next by the invention of a practical substitute for uniform inductance—coils at intervals, one per mile for instance". The memorandum points out that the cable companies were free to explore the matter, for he says: "I did not protect it. There can be no patent for it in England except for improvements thereon. . . . Now the wireless telegraphy frightens them. Well, I wish they may have good reason to be frightened."

Concerning the prospects for wireless telephony, he expressed himself, on April 3, 1914, as follows:

> The atmospheric disturbances will be very troublesome perhaps. Still the idea of talking from pole to pole is rather attractive, or to hear yourself talk 24,000 miles.

The sequestration in which he existed robs the biographer of the ordinary means of approach to knowledge of him. Yet his seclusion, like that of Barrie, was comparatively well known. Fortunately, the opportunity occurs to disclose for the first timé some of his correspondence. The introduction of extracts from this source will disturb continuity, but there is compensation for a broken story in a new impression.

It is first necessary to piece together some scattered facts relating to electromagnetic wave history. The experiments of Bezold (*Berichte der Bayrischen Akad. d. Wissensch.*) on electric waves along wires were described in 1870. They were then unknown to Heinrich Hertz, and they were unknown to FitzGerald (born August 3, 1851; died February 22, 1901). On November 17, 1879, FitzGerald (Fig. 9) read a paper before the Royal Dublin Society on the possibility of originating wave disturbances in the ether by means of electric forces. He there directed attention to Maxwell's statement of the hypothetical conditions under which an electrical disturbance might be propagated in free space. FitzGerald at that time (1879), taking a case of insufficient generality, arrived at the wrong conclusion that Maxwell's displacement currents, however

produced, will "never be so distributed as to originate wave disturbances propagated through space outside the system". In May, 1882, however, after referring to a more general solu-

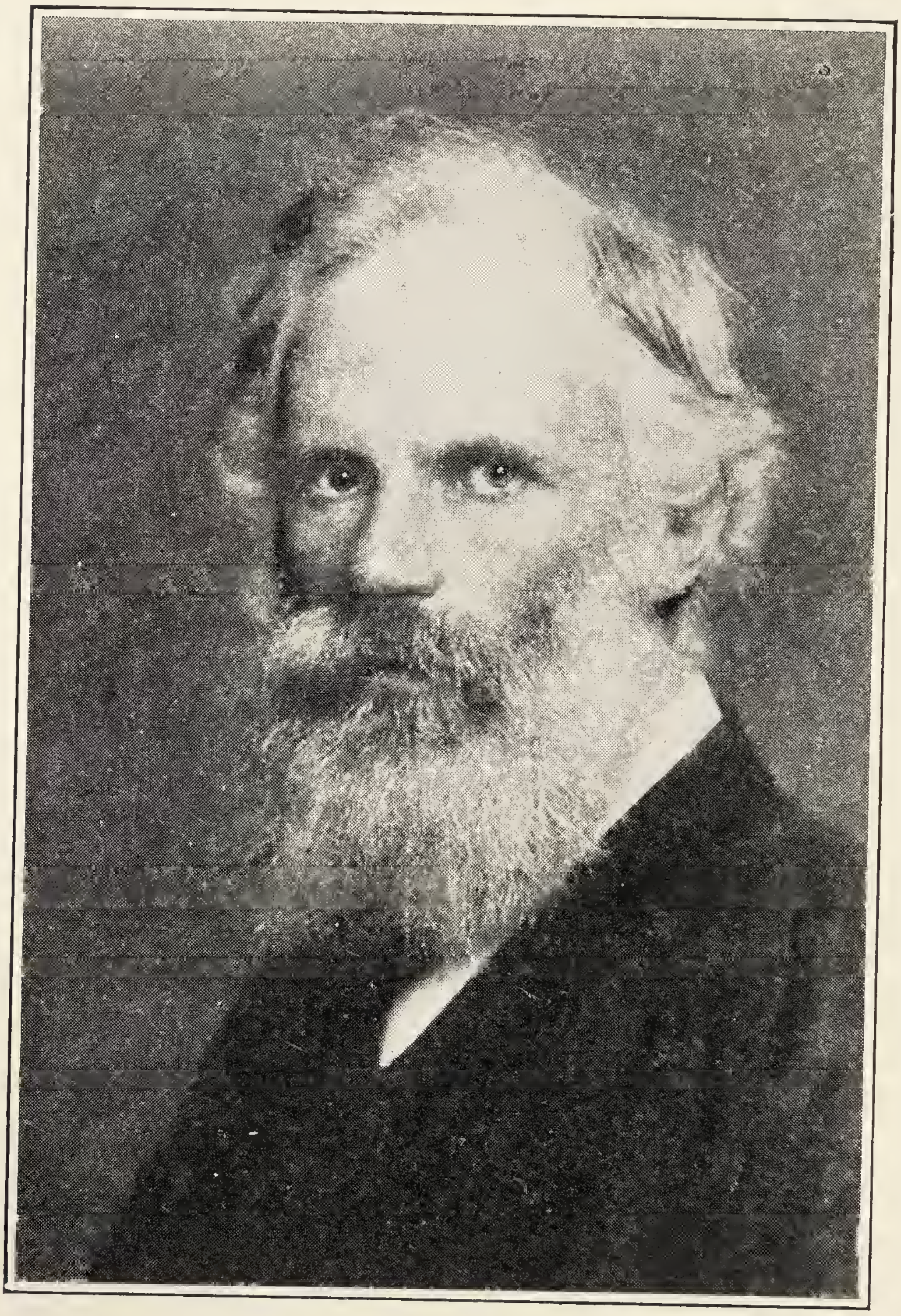


Fig. 9. Professor George Francis FitzGerald.

tion of Maxwell's equations, given by Lord Rayleigh, he withdrew his earlier expression of opinion, and he admitted that "a simply periodic current would originate wave disturbances such as light". He added that it might "be possible to obtain sufficiently rapidly alternating currents by discharging con-

densers through circuits of small resistance". The crucial experiment of Hertz was in 1886.

It was FitzGerald who reviewed, in 1893, Heaviside's *Electrical Papers*, and who acknowledged their value

> . . . because they teach a sound theory of telegraphs and telephones, and of other matters . . . which, there is every prospect, may lead to vast improvements in telegraphy and may even make it possible to work a telephone across the Atlantic.

Regretfully this learned and sympathetic reviewer complained that Heaviside jumped deep double fences and introduced short-cut expressions that were woeful stumbling-blocks to the slow-paced average man; he criticized also the frequent repetitions, and he never forgave him for abandoning quaternions. Nevertheless he ascribed to Heaviside the credit for having cleared away the débris of the battle fought by Maxwell. Heaviside, he observed, had reduced the maze of symbols—electric and magnetic potential, vector potential, electric force, current, displacement, magnetic force, and induction—practically to two, electric and magnetic force. He had established symmetry throughout the whole of electromagnetics. He had extended the theory of wave-propagation in complicated media; and he was responsible for the most important application of electromagnetic theory to telegraphy and telephony. Then comes the highest tribute of all:

> Since Oliver Heaviside has written, the whole subject of electromagnetism has been remodeled by his work. No future introduction to the subject will be at all final that does not attack the problem from at least a somewhat similar standpoint to the one he puts forward.

All creation, in 1886, awoke from its lethargy and became conscious of the existence of electromagnetic waves. Men to-day who remember the delight with which the convincing experiments were at last seen, discussed, and repeated will recall the pleasure of discovery that in 1886 pulsed through the world. It was a victory in which all rejoiced. By doing full justice to Bezold, Hertz won more than fame. His attitude, equally correct, towards Heaviside can now be traced; for

amongst correspondence just made available are letters that tell their own story. The first, from Hertz at Karlsruhe to Heaviside in Devonshire, is dated March 21, 1889:

I more clearly understood your methods from your letter than from your book, where they lie hidden beneath a great number of special cases. I am quite of your opinion, that you have gone further on than Maxwell, and that if he lived he would have acknowledged the superiority of your methods. A great point is, I think, that you do away with unnecessary potentials. Electrostatical (scalar) potential and magnetical (scalar) potential ought to remain I think, but in statical phenomena only; in dynamical problems no potential ought to occur, and no vector potential ought to occur at all. I had also reflected on these things. . . . I find it so very difficult to follow your symbols and your very original mode of expressing yourself. You know mathematical symbols are like a language and your writing is like a very remote dialect of it. . . . Your methods are more than your symbols. . . . I was very interested to hear you had come so near to see yourself electromagnetic waves, and was glad for my sake you did not follow up the indication you had. I cannot quite agree with what you write about the propagation of spirals. I cannot but think that from a good theory quite a distinct velocity ought to come out, and very simply.

If Maxwell lived, I think he would have more joy in my experiments and would have more reason to be proud in their result than I can have.

The reply is unknown, but on May 5, 1889, Hertz continued:

The fact is that the more things became clearer to myself and the more I then returned to your book, the more I saw that essentially you had already made much earlier the progress I thought to make, and the more the respect for your work was growing in me. But I could not take it immediately from your book, and others told me they could hardly understand your writing at all, so I felt obliged to give you warning that you are a little obscure for ordinary men.

Again, on August 10, 1889, now from Bonn, Hertz wrote:

Theory goes much further than the experiments, for the experiments hardly come to tell in a whispering voice what theory tells in clear and loud sentences. But I think in due time there will come from experiment many new things which are not now in

theory, and I have even now complaint against theory, which I think cannot be overcome until further experimental help. You speak of calculating the frequency of such oscillating systems as I make use of. I often tried to get the oscillating time exactly after Maxwell's theory but did not get any. I then considered a simple sphere perfectly conducting in a perfect dielectric. There were no more difficulties in the analysis, but yet I got no more oscillating time. I think there is none. Did you ever work out the problem completely? To my great grief, I have no time to go further on in these things for a year or so, having to spend too much time with my lectures, laboratory, examinations, etc.

To this, Heaviside probably responded promptly, for on September 3, 1889, from Bonn, Hertz explained:

. . . As to the oscillation or oscillations of a sphere, I attacked the problem just in the way you propose but got no result. I think the damping is so great that disturbances go away almost aperiodically. This ought to be otherwise in very elongated ellipsoids, but in these the analysis becomes very difficult. . . . You may believe that I was fully in earnest when I said you could not learn very much of my experiments. I meant to say, that he who was fully convinced of the truth of Maxwell's equations and was able to interpret them, did know as much about these things before my experiments as after them. I did not mean to say by this that the experiments were of little worth, for there were many people not convinced of those equations or not able at all to see what they meant. And then I hope for many new things to come from the experiments. . . . The motion of the ether relatively to matter—this indeed is a great mystery. I thought about it often but did not get an inch in advance. I hope for experimental help; all that has been done till now has given negative results. . . . Take a copper sphere rotating in a homogeneous magnetic field. You cannot treat the case without having recourse to action at a distance. Maxwell's solution is by action at a distance. And I do not see how it could be otherwise before we know if the ether turns round with the sphere or is at rest, or where is the frontier between the moving and the resting ether. . . .

As to the structure of the ether, . . . the structure of all the models imagined until now is certainly not the structure of the ether; in these points I am absolutely of your opinion.

In this letter Hertz enumerated the particulars in which, in his judgment, Maxwell's theory required to be amended:

. . . for example the strains imagined by Maxwell to account for the motions of ponderable matter—these strains would tend to give motion to the interior of the ether itself, except in the very special case of statical problems. Now if no such thing occurs (which I think probable) the system is false; or if such a thing occurs, the system is incomplete.

Though Hertz and Heaviside never spoke face to face, their friendship deepened sufficiently to enable Hertz to write from Bonn on December 31, 1890:

Dear Heaviside,

I send you my very best wishes for a happy New Year. . . . If you would only take a good form, a book of yours on the theory of electricity would have a great success in England and abroad. But I fear you have some pride in this, not to yield to the understanding of others. I think this is a false pride; you certainly are not aware how very difficult your papers are to understand to others, and it is old wisdom that the many will expect you to come to them and not come up to you, be your merits ever so great.

Thus it was not for lack of good advice that Oliver continued to indulge in the luxury of his own obscurity. Towards Hertz, he had the kindliest feeling. He concluded that, so far as the ether, as distinct from matter, was concerned, the Karlsruhe experiments fully confirmed Maxwell's theory. Matter would require further consideration. Maxwell's application of electromagnetics to explain phenomena associated with waves of light and heat in solids and liquids he knew to be imperfect, but the best available.

There is strange irony in the fact that Heaviside's secluded home in Devonshire, half buried amongst its brambles, its doors closed to all save the rarest visitors, its interior too often comfortless, should at the same time have been to a large section of the most advanced men of science a temple of wisdom, the place of the oracle, the court of ultimate appeal. The proof of this is in his correspondence. Physicists at Oxford who wrote to him referred occasionally to his hermitage as "The Inexhaustible Cavity", and there is a story that a letter addressed to him at "Inexh. Cavy. Torquay" was duly de-

livered. The reference, no doubt, was to the cave of Adullam (1 Samuel xxvii., v. 2) into which everyone gathered that was in distress. Sometimes the perplexing problems sent to him were relieved by lighter matter, such as the following from C. V. Burton:

Crookes (P.R.S.) was dined at Jesus Coll. Oxford a little time ago. All sorts of "nutts" came to meet him, but he spoke hardly a word to anyone. According to one account, he has found that if he talks too much after dinner he makes no new discoveries the next day.

There was a desire to prevail upon Heaviside to clear up the distinction between his vectors and quaternions, and to establish harmony if not coincidence. He held, however, that:

As vectors are not quaternions, the algebras cannot be naturally the same. Quaternions should come into vector algebra as quaternions—not as vectors. . . . Quaternions belong to trigonometry, which is the science of ratios between differently directed vectors. . . . Vector calculus belongs to physics.

The playground remained covered—as ever, since Descartes—with broken models of the ether. One of the first to acknowledge the wisdom that emanated from the inexhaustible cavity was Sir Joseph Larmor, who confessed in a letter dated October 12, 1893:

I am practically a convert to vector analysis and I mean to learn up the machinery immediately I have time.

Later he added:

My present view is that atoms are vortices in the medium, then magnetism is their vorticity, and a magnetic field tends to align them.

On February 12, 1898, he sent Heaviside "to look at" a copy of his paper on rotational ether, making the electric field of an electron both polar and circuital at the same time, in a way "that carried some evidence of naturalness". It had cost him three years to prepare, and yet he declared, "I am not so sanguine as to hope that more than say two people will read

it". A few weeks later, Heaviside was able to send him vol. ii. of *Electromagnetic Theory*. For this Larmor, on May 8, 1899, thanked him, observing that it contains

> . . . much wholesome castigation of my own profession, which is misled sometimes by the power of symbols into the belief that such a thing as rigorous exactitude can reign in any created product of the human mind. I see you still decline to countenance a rotational singularity or electron. . . . I believe I see signs that some previous oft heard opponents of all such notions now exhibit a tendency towards taking them for evident. But that is the way of the world, as you I presume are aware.

In 1898, Heaviside was consulted by John Milne with regard to seismographs and the interpretation of seismographical curves. Milne was prospecting and negotiating for a volcanic site near to Heaviside, on which to establish an observatory; for, according to Milne,

> If the National Laboratory is to be at Kew, so far as certain kinds of work are concerned it might equally well be on a sponge.

But the site Milne had prospected and coveted turned out to be not volcanic after all.

The gathering strength of the influence of Oliver Heaviside upon his contemporaries is everywhere apparent in the correspondence. Amongst the letters, for example, is one from David Hughes, dated May 19, 1899:

> I should like to ask you if I am wrong in supposing that theory leads to the conclusion, 1st that self-induction is beneficial to long-distance telephony, and 2nd that you would recommend for that purpose *Iron wires in place of Copper*. I mean wires of the same resistance.

There is another from David Hughes, dated June 6, 1889:

> Remember that I fully agree with Hertz's experiments and think Maxwell's electromagnetic theory of light probable—but the only point in doubt in my mind is the permeation of a current from the inside to the outside. . . . If you can cite any experiment or any reasoning that would guide me in finding a method by which it could be demonstrated, your reply would be valuable to scientific truth.

Complete triumph came on January 10, 1889. The date is noteworthy because it is that upon which The Society of Telegraphic Engineers and Electricians altered its name to The Institution of Electrical Engineers. After the transfer formalities, Sir William Thomson (Lord Kelvin) took the Chair and gave an address on "Ether, Electricity and Ponderable Matter". He dwelt upon the history of electromagnetic induction in cable transmission, and he agreed that in his early theory he had not taken it into account at all:

> But in the meantime it has been worked out in a very complete manner by Mr. Oliver Heaviside; and Mr. Heaviside has pointed out and accentuated this result of his mathematical theory—that electromagnetic induction is a positive benefit: it helps to carry the current. It is the same kind of benefit that mass is to a body shoved along against a viscous resistance.

The question of the choice of metal for conductors where "impulsive current" is concerned was discussed at a meeting of the Institution of Electrical Engineers on May 9, 1889, when Dr. J. Hopkinson was in the Chair. It was on this occasion (*Jour. Inst. Elec. Engrs.*, vol. xviii. p. 497) that Professor D. E. Hughes declared that "he could not agree with the theory of Oliver Heaviside if it leads to the conclusion that iron for telephone wires is better than copper". Dr. Hopkinson intervened and pointed out that Heaviside was very guarded in the statement, and had said "that an arrangement of iron was suggested which might be an improvement, but the ordinary iron wire was recognized as being very inferior in practical conductivity to copper wire. . . ."

On June 26, 1893, an editor wrote that John Perry had been for a long time wrestling with the subject of telephoning through cables, and "felt" that Heaviside's conclusions were right but could not follow the working. It was remarks of this kind that elicited from the "cave" the observation that the new theory "is so obscure that it has attracted a good deal of attention". Perry realized that if Heaviside would write a book that could be easily understood "it might pay", and he thought that he himself would be a good "foolometer". From a letter of FitzGerald (February 2, 1894) it appears that Perry would have

been glad to collaborate with Heaviside in this venture. The precise opinion Perry held at about this time is recorded in a letter (August 4, 1893) to a mutual friend:

Now I rank Heaviside with those two men (Kelvin and FitzGerald) but I never pretend to be able to read Heaviside. I wish I could, and so do a lot of people like me. . . . Somebody will have to write down Heaviside to our level.

With refreshing candour, ever associated with John Perry by those who knew him, he wrote to Heaviside (March 4, 1896) his confession:

I only dip into your work to take out what is useful to myself. I only know you to be the great man you are through him (FitzGerald).

An important part of the task of freeing the works of Heaviside from their obscurity, of enhancing their value by numerical examples, and of inspiring them with new life directed towards practical achievement, was reserved for Henry Malcolm, who in 1912 began and in 1917 completed his classic treatise, *The Theory of the Submarine Telegraph and Telephone Cable.*

The correspondence of Heaviside reveals his esteem for FitzGerald, who, as he expressed it, "belonged to the class of first-rate scientific men of the profounder sort who, not having any pretensions, only become known to their contemporaries". Concerning scientific men of the contrary type he observed: "There is no need to go without fame if you really want it". It is appropriate therefore to take the opportunity that now for the first time presents itself of making known some of the opinions and criticisms that passed between these two remarkable philosophers. FitzGerald, at Trinity College, Dublin, began by tilting at Heaviside's innovations. He wrote to him:

(*September* 26, 1892.) I hope you will succeed in making the ordinary mathematical physicist think in vectors, though I do not think your notation an improvement. You see, I was "riz" on Tait and get very much muddled by your omission of S; and when one gets bothered every turn one naturally takes a dislike to the botheration. . . .

(*January* 4, 1894.) Trouton took up the telephone question rather by the way and has gone off on what he was at before, when he found it so complicated and unintelligible.

(*March* 15, 1895.) You say Maxwell's (magnetic theory) is all a muddle. So it is: but there is an underlying stratum of explicability, I think, which must have been unconsciously guiding him.

(*June* 21, 1893.) Maxwell's electrokinetic moment, A, was a crude way of imagining that the *current* had all the inertia, which is quite out of date now thanks to you—but it was very well when he wrote. . . . I cannot say that I am quite satisfied with your suggestion that in an imperfect conductor there are local electric currents with accompanying magnetic inertia as a way of getting over the statement that all the inertia is magnetic. It does not diminish the necessity for taking account of some inertia besides that appearing in the form of magnetic inertia round the current, because *wherever* a current exists some local energy is degraded *in situ* and if this has accompanying inertia without an external field we ought to add on to all our currents a term to express this. Just as the magnetic field due to two parallel wires cannot be completely supposed due to one wire between them carrying the sum of their currents, so, if we have to take account of internal fields between the molecules, we should proceed to do so, and *one* way of *representing* it would be in the *form* of inertia of current.

As regards the question of electric energy being potential, and magnetic kinetic, that is not in question here. When a spring is bent its energy is potential and still there is kinetic energy involved *during the time of its being bent*. What seems to me most likely is that during the time that displacement is taking place there are two sets of things going on. There is a magnetic force and accompanying induction (flow) taking place all round the displacement current which is proportional to the rate of change of the displacement and is the seat of *almost* all the kinetic energy. But besides this there is the change in structure of the medium going on which we call electric displacement and which is *increasing* as long as the rate of change continues constant; *i.e.* there is something increasing while the magnetic flow is constant. This is proved by the fact that there is electric force developed tending to push back the electric displacement. Now this something that is increasing must be some other motion than the magnetic flow, for magnetic flow cannot exist without calling up any reaction of the kind considered, and it seems to me as if this something increasing must have some inertia, for *no* change can take place without *some* inertia somewhere, and if this change *is* different from the magnetic

flow then there *is* some inertia, maybe very little, in addition to the magnetic flow.

I think, however, there is a possible suggestion that what free ether resists is curl of magnetic flow, and then of course, the inertia of the flow would not differ from that of its curl—this latter being only a particular distribution of flow. But even then the *curl* is not increasing, it is constant, so that even though it is resisted, how can the resistance increase with the time? That is, I fear, special pleading, because the flow itself being a changing thing, its curl is so too. However, then comes the difficulty about what is happening between two spheres while the electric displacement is changing. There is no magnetic flow, therefore no curl, and so the curl of magnetic flow cannot be the something that is elastically resisted, unless of course you consider how the displacement *begins*; and in a non-conductor there must be the current to the inside of the sphere *somewhere* and there is accompanying curl somewhere; and from this point of view it is interesting to see *how* the electric energy grows at the various points within the sphere as they are gradually deserted by the magnetic force leaving its integral curl behind. Is that possible? I am afraid thinking these things out on a sheet of paper thus leaves them very muddly.

As to electric energy being essentially kinetic as well as magnetic energy I have no doubt, but agree with you in thinking that provisionally it helps matters to consider one of them as kinetic and the other as potential. I return to my difficulty. Even in the case of curl being resisted, curl to be resisted must mean some changed structure other than uncurled flow, and this changed structure will have some accompanying inertia. Final thought: But perhaps different from the curly inertia.

I am afraid I am very hazy myself and am working more or less on my own model ether for concrete ideas, so that I am afraid I cannot make it very clear to another.

(*June* 21, 1893.) When a current is started near a permanent magnet, the energy spent in producing it is quite independent of the presence of the magnet: Whence then in this case comes the energy of the field? I can only suggest that the permanent magnet is like a coil of a practically infinite self-induction, and that the energy of the field comes from it as it would from a neighbouring coil of perfect conductivity.

(*August* 22, 1893.) Thank you very much for your letter. It clears up what I was quite hazy about. I had forgotten the momentum given to the ether, though I had been rather harping upon it lately in the case of the radiation of waves. . . . There is

merely radiation of energy from two electrified bodies rotating round one another. There merely is a system of corkscrew waves being constantly generated . . . any regular periodic disturbance confined within a finite space must be continually creating new waves. . . . The action between two vortex rings in a perfect liquid is not . . . *propagated* from one to the other. It is due to the fact that each vortex ring is accompanied by motion everywhere, *i.e.* each vortex ring is itself really infinite and each ring involves all the others. Effects due to this kind of action not being propagated are simultaneous everywhere, and—as some actions of this kind must, I think, exist—it seems to me extremely probable that gravity is *the* action of this kind we are looking for. In that case there would be no question of its rate of propagation, it would be due to the fact that each atom of matter is infinite, *the* most probable hypothesis possible.—Yours sincerely,

Geo. Frs. FitzGerald.

In March 1896, FitzGerald wrote to express his satisfaction that Heaviside had accepted the Civil Pension awarded in recognition of services to his country. He attributed the negotiation of the matter chiefly to John Perry and to Lord Rayleigh. An announcement that probably caused far more merriment in the "Inexhaustible Cave", however, was made by FitzGerald on June 8, 1896:

I had a long correspondence with (Lord Kelvin) and his last letter says that he gives up everything he ever wrote about the ether. I hope he is not still quite so down in the mouth about it.

This was followed, on May 7, 1898, by a note to the effect that:

Lord Kelvin, with his usual impetuosity has rushed at a single fluid theory of electricity—but he is very rash.

Formal correspondence was followed by a visit, of a few hours, of FitzGerald to Heaviside at Torquay and of cycle rides with him. They were men of such presence that those English lanes must have thought King Arthur and one of his Knights passed again to Camelot. The scene too quickly changed to Dublin, whence FitzGerald wrote:

(*May* 7, 1899.) Have you worked at the propagation of waves round a sphere? A case of this is troubling speculators as to

the possibility of telegraphing by electromagnetic free waves to America. It is evidently a question of diffraction and I think must be soluble. Perhaps the case of propagation round a cylinder would be easier. . . .

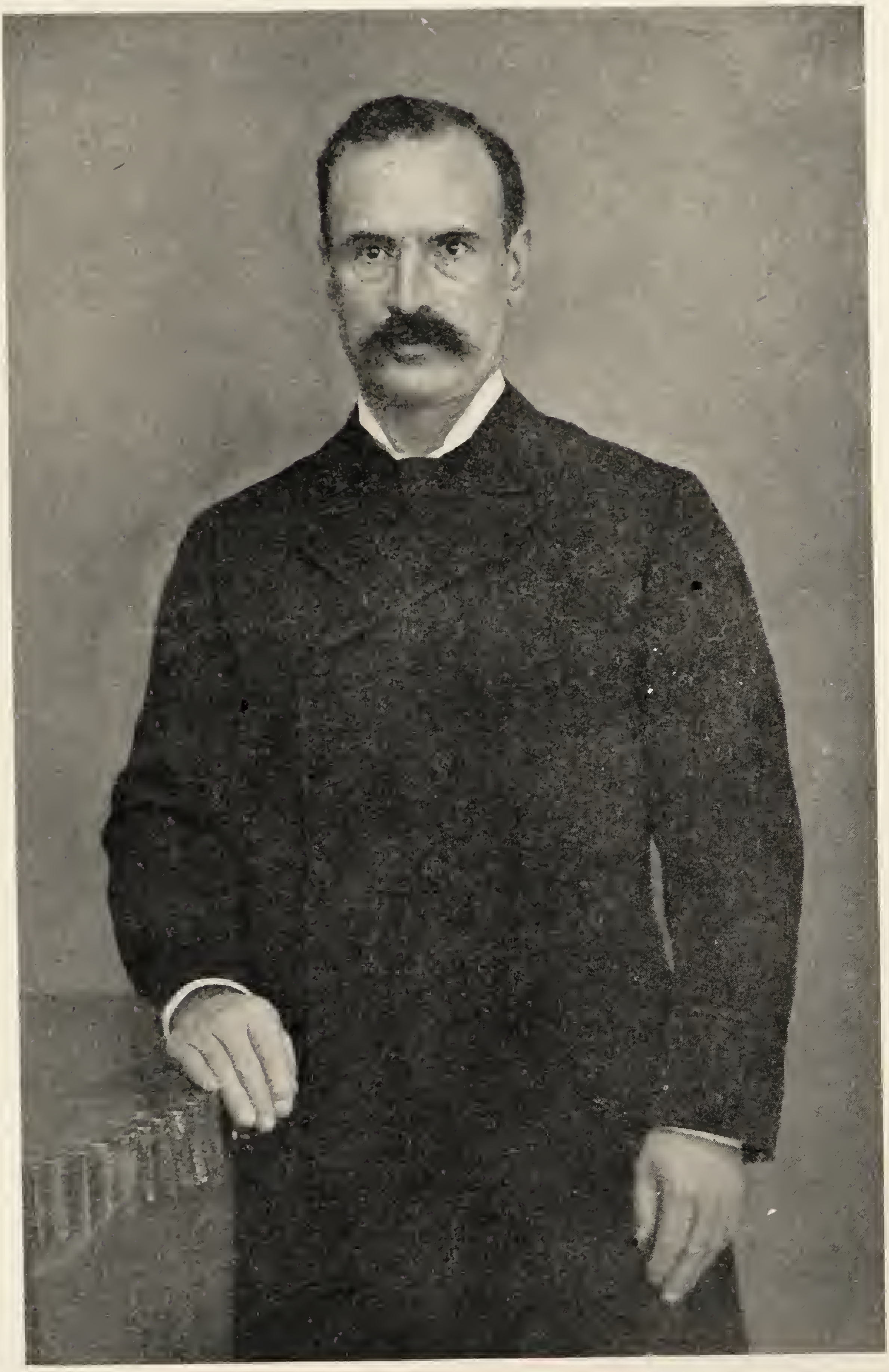
(*September* 21, 1899.) I am delighted to hear that you have set up that Æolian. You are so fond of music I am sure it is very good for you and will help you to do more and better work than you could ever have done without it. . . .

By this time the authoritative character of utterances from the oracle was recognized even by so distinguished a literary purist as Dr. Murray of Oxford, who, on March 17, 1899, appealed for the derivation of "Impedance". In reply, the lexicographer was referred to:

The Electrician, July 23, 1886, p. 212; *Electrical Papers*, vol. 2, p. 64, and to the quotation—"Let us call the ratio of the impressed force to the current in a line when electrostatic induction is ignorable, the Impedance of the line". . . .

Amongst the few contemporaries capable of appreciating to the full the merits of Oliver Heaviside was George Minchin, who, from Trinity College, Dublin, had been appointed Professor of applied mathematics at the Royal Indian Engineering College, Coopers Hill, near Windsor. Mathematician, physicist, poet, and lawn-tennis player, he excelled in all that he set his mind and hand to do. He was a pioneer in photo-electricity and an early enthusiast in wireless telegraphy, especially in the use of an antenna; his skill in expressing in clear terms mathematical conceptions was unexcelled, and his joyous outlook made his friendship one of the best possessions. FitzGerald visited him at Coopers Hill College and wrote from there to Heaviside. This, so far as can be traced, was the last of their communications; for FitzGerald died but a few months later—died, as did Maxwell and Hertz, before reaching fifty years of age.

(*July* 12, 1900.) I was fortunate in being with Larmor in Cambridge when your letter was forwarded to me, and so I asked him about the difference between himself and your work, and he pointed out at once what would have taken me some thought to discover. It all arises from the difference he takes between a *moving electron* and a changing displacement. The electron is certainly a change of



George M. Minchin, F.R.S. (1887), Professor of Mathematics at the Royal Indian Engineering College, Coopers Hill.

place of the point, but we cannot say that the changing displacement is a real motion in the direction of the displacement. The electric displacement at a point is, no doubt, represented by a vector, but it is very unlikely that it is really a simple displacement of the point: it is much more likely to be a rather complex change in the structure of the ether at the point, which can be *represented* by a vector. In consequence of this difference, Larmor separates the electric force, which acts on the ether and produces the electric displacement, from the force on a moving electron due to its motion across a magnetic field. When matter moves across the ether in which there is magnetic force, this latter is what produces the electric current, *i.e.* a current of electrons. Its value is

$$V\dot{\rho}H$$

(V in FitzGerald's notation here represents vector product) while there is no electric force producing any displacement of the ether due to the motion: unless the induction changes owing to the moving matter and so produces an electric force that acts on the ether.

Larmor, working with his abominable potentials separates them as

$$\epsilon_1 = -\frac{dF}{dt} - \frac{d\phi}{d\eta}$$

$$\epsilon_2 = \dot{y}c - \dot{z}b$$

and the ether being considered as standing still ($\dot{x}$, $\dot{y}$, $\dot{z}$) can only refer to moving electrons. The two together produce the *displacement* which, when changing, is partly changing ether displacement and partly current of electrons, and are accompanied by the magnetic force,

$$\dot{D} = V\Delta H$$

I think there is very great difficulty in deciding what is the best assumption to make as to the interaction of ether and matter when they are considered, as you consider them, as *continuous* interpenetrating media. Larmor, by assuming a definite hypothesis as to the nature of matter and its connection with ether by means of electrons (is enabled) to decide which of different suppositions is best.

I have to go off now to help Minchin to put up some wireless telegraph poles. I was very sorry indeed to hear of your (bicycle) accident with the hen. Hope you are getting over it all right and that it won't make you afraid to continue to cycle.—Yours sincerely, GEO. FRANCIS FITZGERALD.

In the introduction to the *Collected Scientific Writings of*

FitzGerald, there is a letter from Heaviside—the real Heaviside, at his best—disclosing very tender feeling, written at the time of FitzGerald's death:

I only saw him twice knowingly, once for two hours, and then again for six hours, after a long interval; yet we had a good deal of correspondence at one time, and I seemed to have quite an affection for him. A mutual understanding had something to do with that. You know that, in the pre-Hertzian days, he had done a good deal of work, not large in bulk but very choice and original, in relation to the possibilities of Maxwell's theory, then considerably undeveloped and little understood; and his way of looking at things was more like my own than anybody's. Well, he found that I had done a lot of work in the same line, and he was most generous in recognizing and emphasizing it. Too generous, of course . . . he used to write to me a good deal about electromagnetic problems, and I laid down the law to him like—like myself, in fact. He took it all very pleasantly. But I knew all the time that he had a wider field than myself, and no time to specialize much.

Between the years 1855 and 1912, during which the theory of electrical transmission through telegraph and telephone cables was built up, the three electricians chiefly responsible for advance in that theory were Maxwell, Kelvin, and Heaviside. Heaviside's appreciation of Maxwell is manifest. Although his acknowledgement of Kelvin's early work is definite, it was not until the year 1889—when Kelvin acclaimed Heaviside—that their acquaintance with one another began to glow. There are two letters from Kelvin to him that serve to illustrate this:

(Glasgow University, *November* 4, 1888.) Dr. Francis forwarded to me at Cambridge your letter with accompanying papers. I sent him back immediately the papers for publication in the *Phil. Mag.* but I don't agree that velocity of propagation of electric potential is a merely metaphysical question. Consider an electrified globe, A, moved to and fro, with simple harmonic motion if you please, to fix the ideas. Consider very quickly acting electroscopes B, B', at different distances from A. If the indications of B, B' were in exactly the same phase however their places are changed, the velocity of propagation of electric potential would be infinite; but if they show differences of phase they would demonstrate a velocity of propagation of electric potential. Neither is velocity of propagation of "vector potential" metaphysical. It is simply the

velocity of propagation of electromagnetic force—of "electromagnetic waves" in fact.

(Glasgow University, *April* 27, 1899.) I am not bigoted to either "spin" or "rot" or "turn"; but I have always thought some of them better than "curl", as curl seems to me to involve the idea of either a helix or a flat spiral. I see I was wrong in attributing "curl" to Clifford. He gives a good many such words, but it was, as you say, Maxwell that first gave *curl*, as he in fact tells us himself in the first volume of his *Electricity and Magnetism*. It is rather the symbolic system connected with it in your own and Maxwell's papers that I object to, than the word itself, and I cannot agree with any attack on Cartesian co-ordinates. All words that help us out of aphasia, provided they promote clearness instead of the reverse, are to be welcomed. . . . We want a thorough mechanical theory which shall include the undulatory theory of light with electrostatics, and electromagnetic force, and electromagnetic induction, with the *mobility of the medium and all the bodies concerned*, which is part of the essential nature of the affair.—Yours very truly, William Thomson.

The problem of calculating the effective resistance of the inner conductor of a concentric cable was approached by Maxwell (*Electricity and Magnetism*, vol. ii. p. 690) for low frequencies, and only for a few terms of a series. Heaviside considered the whole "throttling" effect, *i.e.* the effective resistance, the effective inductance, and the tendency to surface concentration (*The Electrician*, May 3, 1884, p. 583), using two functions, M and N, which Kelvin later called the "ber" and "bei" functions. Heaviside developed this study (*The Electrician*, January 3, 1885), and Alexander Russell elucidated and extended it in a valuable review of the state of knowledge of the matter (*Proceedings of the Physical Society*, vol. xxi., part vi., December, 1909).

The date of an event of some consequence—a visit by Hertz to England—is recorded in a letter written on November 24, 1890, by Professor Ayrton.

Dear Mr. Heaviside,

Professor Hertz is coming to stay with me for two or three days at the end of this week, and on Sunday some friends who are interested in electromagnetic radiation are coming to lunch with us to meet Professor Hertz. If by chance you

will be in London Sunday next the 30th inst., we shall be delighted if you will come to lunch with us.—Sincerely yours,

W. E. Ayrton.

It is safe to assume that Heaviside was not present. Three years later Ayrton wrote to him for a definition of inductance, and on January 25, 1896, he again asked him for assistance in calculating the size of a copper plate to represent, in metal, the sea. At that time an endeavour was being made by electricians, who consulted Ayrton, to estimate the practicability or otherwise of telegraphy from the shore to a lightship. They knew that, owing to the swinging of the ship with the tide, a cable could only with difficulty be taken on board. The plan was to place a large coil at the bottom of the sea. Through the coil an alternating current was to be sent from the land to act inductively on a similar coil in the ship. Ayrton also inquired of Heaviside how the current in the coil would be affected by the sea.

In 1897 Heaviside was urged by a representative of the Northern Lighthouse Board to examine the proposal to utilize an induction coil and telephone in lightships to pick up signals from a corresponding coil at the bottom of the sea.

On March 8, 1905, a Cambridge mathematician wrote to him for a proof of the now familiar conjugate theorem that if a current, I, entering a solid object at A and leaving at B produces a difference of potential, V, between two other points, M and N, in the body, then a current I entering at M and leaving at N produces a difference of potential V between A and B.

His views on electrical matters were also requested by correspondents as distantly scattered as Mandalay, Calcutta, and Allegheny.

The knowledge possessed by him of phenomena relating to submarine cables, and his profound study of mathematical principles relating thereto, enabled him to impart new life to a world of electrical communication that, in essentials affecting construction, had remained, for about thirty years, dormant. His advocacy of "loading" was not at first received with favour in his own country. For telegraph cables, Kelvin—recognizing

the analogy between electrical transmission and the diffusion of heat, examined by Fourier—had developed the "K R" law and had shown how to draw arrival curves for the case of a cable devoid of inductance and leakance, when both ends are put direct to earth. It was left to Heaviside to discover how to take account of inductance and leakance and how to develop equations to predetermine the effect upon the arrival current of the insertion of condensers and other apparatus at one or other or both of the ends.

It was also left to Heaviside to discover that there is a critical relationship between the four cardinal quantities—resistance, capacity, inductance, and leakance—in any telegraph or telephone circuit, and that, when this is fulfilled, received signals are an exact reproduction of those sent. He called this the "distortionless" condition—for his onomastic skill was unexcelled. He compared the case with that corresponding to greater inductance, and he exemplified the effects of reflection. His proposals for effecting improvement by adding inductance continuously, or in "isolated lumps", are described in *Electromagnetic Theory*, vol. i. pp. 444-46, where he sets forth his work of 1886–87.

There is a marginal note in pencil on a stray reprint, that when he first suggested loading at a meeting of the Physical Society, a well-known physicist, Blakesley, at that time said, "it would be like making humps on a road to increase the speed of vehicles".

In common with all pioneers, his mind turned at last to the question of the structure of matter; here are some of his thoughts about it, so far as is known, unpublished:

> There are wheels within wheels, and the elementary volume itself may be a highly complicated dynamical structure with various sorts of energy in it, determinately connected with the external world. Nor is this mere mathematics. It seems to me that the state of things suggested is very likely that which prevails in the universe of molecules. The usual mathematics of continuous actions through elastic media takes matters in the gross. The unit volume must be large enough to contain an enormous number of molecules. The first approximation to a molecule is a little lump of matter banging about, exchanging its momentum and energy with

its neighbours. But the molecule itself may be a little world, and on magnification, its affairs may be as complicated and important as those in our world. There is no absolute scale of magnitude in matters of length and time. Those wonders of wonders, thought and reason and memory, probably involve an inner mechanism of the atoms, especially as regards the storage of ideas involved in memory, to be lost sight of for long periods. Extremes meet, and the fast decaying brain of the old man brings to the surface the events of child life. The fact that the brain is subject to material change and replacement during life does not debar the theory of partial dependence upon the inner world of the atom. The replacement tends to follow established lines more or less perfectly; usually less, of course. We do not want a special kind of "mind-stuff". In any case, I cannot conceive the possibility of such a thing as long continued memory on the lines of mere external chemistry, and averages of molecules. It must depend on something deeper. Carlyle said: "Go deep enough, there is music everywhere." This dogma would perhaps have more truth in it, if for music were substituted "thought". At any rate, it is potentially existent in all matter which can go to make the man . . . how is it that early impressions sink deeper and deeper, becoming harder and harder to recall, and come to the surface again only in old age? It looks as though they worked themselves in deeper and deeper into the atomic mechanism. At any rate, I can construct by ordinary magnetic coils and electrical condensers an arrangement which shall imitate this absorption and subsequent recovery—I do not refer to hysteretic condensers, but ideally perfect ones, the explanation of hysteretic condensers is in fact similar—in a weakened form, returning after many days like the bread that was cast upon the waters. If the inner parts of brain atoms are storage cells for very high frequency waves, if they are emitted they will sympathetically excite similar cells in other brains in an imperfect manner, and so provoke a vague impression, which the thinking part of the brain may develop to a picture. The power of emission may be great in strong mediums; receptivity will be small in a Huxley. Saints' halos! Phosphorescence! Why not? they were funny fellows.

Elsewhere, in a marginal note, he expressed the same idea more concisely, but with less reasoning:

Life is an essential property of matter. All matter is alive, even the deadest. All phenomena are natural phenomena.

Only once amongst his papers is there found any trace of

an attempt to write a story. It is entirely devoid of romance, and takes the form of a sketch entitled "Muscular Characters". It refers to his visits to a public gymnasium at the "Pimple" —presumably Primrose Hill—and his impression of youths who resorted there. And once, only once, is there any trace of his having descended into verse. The occasion was the dedication of vol. i. of *Electromagnetic Theory* to the children of his brother Charles. He speaks of his nieces and nephews as "My dear Children". The verses are written in pencil on the fly-leaf of a copy he presented to Charles:

DEDICATION

TO MY DEAR CHILDREN

1. I did not send you any cards,
   For I had none to send,
   So now I send you this here book,
   Whereby to make amend.

2. The first chap. is for Freddie,
   And may he always be,
   A credit to his parents,
   And an ornament to Torquay.

3. The next chap. is for Ethel,
   And may she read it well,
   And study it, and find it good,
   Nor think the book a sell.

4. The third chap. is for Charlie,
   And may he *never* be,
   A terror to his parents,
   And a torment to Torquay.

5. The fourth chap. is for Rachael,
   Because it is the best,
   And may she never *never* try
   To turn it into jest.

6. The preface is for Beatrice,
   Because it is so short,
   And may she never *never* think
   It all amounts to nought.

7. The Contents are for Pa and Ma,
And may they never know,
The pangs of tortured conscience,
Or the awful depths of woe!

Jan. 1, 1894.

Amongst the unpublished fragments also is this:

Here we may stop to remark on the immortality of the soul. This doctrine, which probably had its first origin in the dreams of savages, survives all attempts to abolish it. In its old-fashioned sense the principle may have already lost its hold upon a great many men and women of the highest attainments, and may be mostly held by those who are least capable of judging, as an article of unthinking and unquestioning faith. But it is part of human nature for all that. When old beliefs are found out of keeping with the spirit of modern knowledge, the proper way is not to abolish them, but to modify their interpretation. Now there is a far nobler sense in which the doctrine of the immortality of the soul is true, not as a matter of faith, but of fact. Everyone in living his life is making the world for those who will follow. Everyone makes some impression on the world, good or bad, and then dies. The good or the mischief he has done remains; the impression is left for all time. Not only the lives of those around us, but of our followers, are modified in consequence of our actions. The aspirant to immortality who is dissatisfied with the old conventions may then cry "Non omnis moriar". A part of us lives after us, diffused through all humanity more or less, and all Nature. This is the immortality of the soul. There are large souls and small souls. The immortal soul of the scienticulist is a small affair, scarcely visible. Indeed its existence has been doubted. That of a Shakespear or a Newton is stupendously big. Such men live the best part of their lives after they are dead. Maxwell is one of these men. His soul will live and grow for long to come, and hundreds of years hence will shine as one of the bright stars of the past, whose light takes ages to reach us.

To remove misapprehension it should be recorded that Oliver Heaviside was neither destitute nor in desperate poverty. Financial troubles came to some extent because, throughout his career, he was devoid of business acumen. He refused much that life had to offer him—even medical attendance in his illness. From several directions his friends approached to offer amelioration. Some of the astutest intellects of his country

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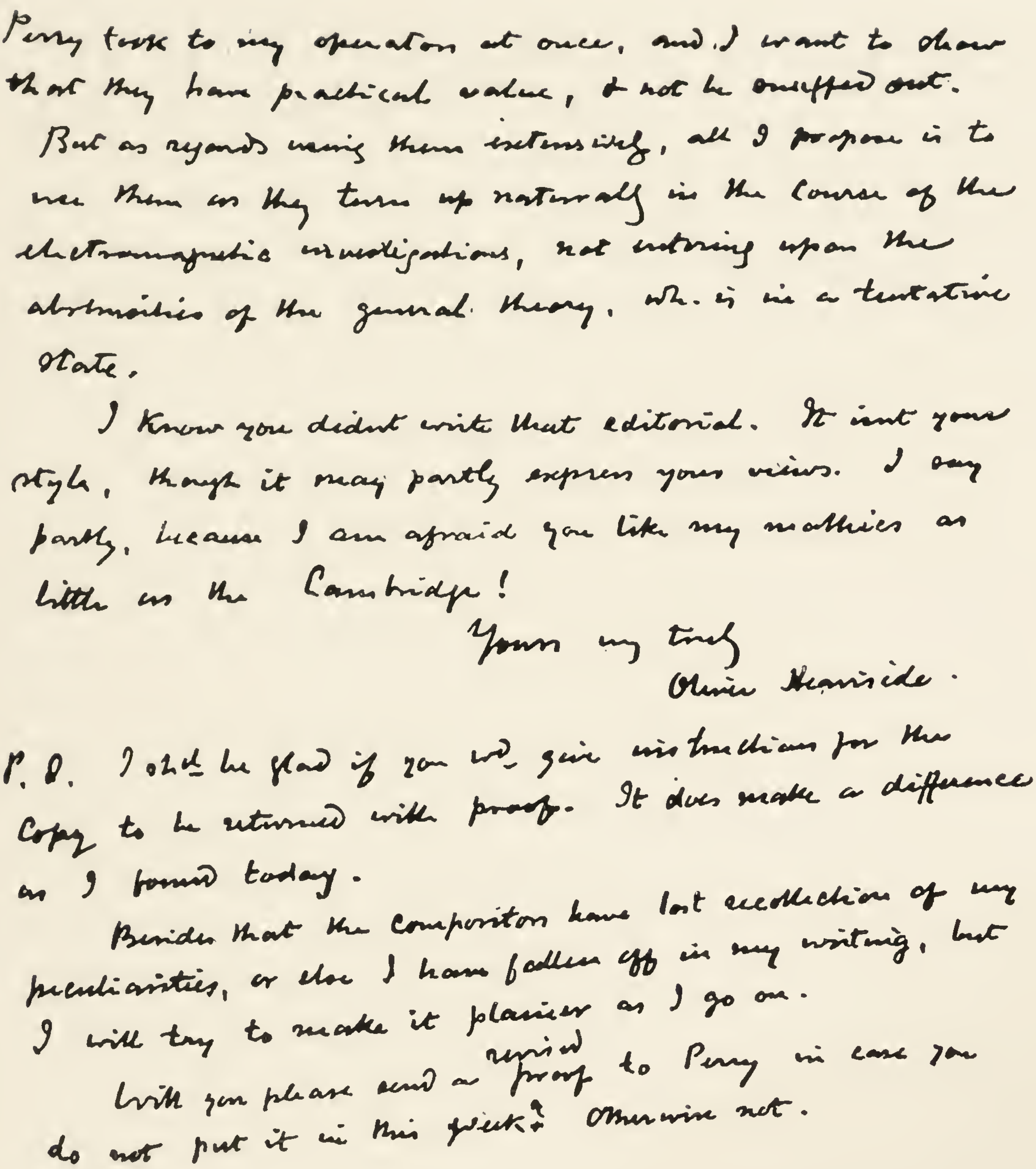
Paignton Dec. 5. 94

Dear Mr Trotter. I almost think that Editors shd be included in the "other wicked people" who would "have it all their own way" At any rate I have compromised as you suggest. It spoils my "prose" though, but I can put it right again for the book. At the same time I may say that the use I made of the word "religion" is a very common one indeed, and that I was careful to put it so as not to offend any pious people by imagining an impossible state of things! I think pious people have it too much their own way too. How about making the first "religion" be "So-called religion", letting the rest stand. But it doesn't matter.

Final paragraph. But then the matter is entirely physical, & is so considered. I wd not condescend to notice any pious theories of the earth's age, not considering them worthy of it. I wasn't referring to pious theories, but to the "moss grown stone", which was chucked into the world once, according to a physicist. That is a physical theory, though possibly it has a pious foundation to account for it in the author's brain. (But my "religion" is also physical, due to physical causes.)

I am a little surprised at your being afraid of the parsons. They dont read The Electrician. Besides, they advance remarkably: look at Haweis, & the stuff he preaches from his pulpit. And the pious people move on too. Things are not now as they were a generation ago, when Huxley & Tyndall were attacked. Besides, no one wd take any notice of me. And finally, I do not attack religion at all, but merely remark on a physical matter.

As regards my sending you this article at all, it arises from the circs. of the moment; viz. Perry's work, which he wont publish, and my work, which I cant get published (at least in the R. Soc.). As it is good both ways, as


Perry took to my operators at once, and I want to show that they have practical value, & not be snuffed out.

But as regards using them extensively, all I propose is to use them as they turn up naturally in the course of the electromagnetic investigations, not entering upon the abstrusities of the general theory, wh. is in a tentative state.

I know you didnt write that editorial. It isnt your style, though it may partly express your views. I say partly, because I am afraid you like my mathics as little as the Cambridge!

Yours very truly
Oliver Heaviside.

P.S. I shd be glad if you wd give instructions for the copy to be returned with proof. It does make a difference as I found today.

Besides that the compositors have lost recollection of my peculiarities, or else I have fallen off in my writing, but I will try to make it plainer as I go on.

Will you please send a revised proof to Perry in case you do not put it in this week? Otherwise not.

FIG. 10. SPECIMEN OF OLIVER HEAVISIDE'S HANDWRITING. This communication was addressed to Mr. Trotter, who in 1894 was editor of *The Electrician*.

conspired to find a way of supplying him with funds in a manner that he might not resent. They succeeded only in part. At the back of his mind was the complexity that he ought to have received from the commercial world early acknowledgement and remuneration for his work in telegraphy and telephony. He suffered, as many an inventor has, to see that the prize had slipped from his grasp into other hands. That being lost, he preferred to live in his own way. Yet one compensating delight

he had: the sense of supremacy in his own domain of mathematical physics. This supremacy was derived primarily from his powers of intuition—exceptionally developed, almost unfailing, and ever inspiring. Heaviside penetrating towards an immortal generality can only be compared with Faraday prospecting for eternal truth in a wilderness of experimental facts.

He detested alcohol in all its forms, but he was an inveterate smoker—a pipe of the strongest tobacco was his delight. At Homefield, in the years 1913–14, he scarcely ever went beyond his garden. If his friends gained admission, they were gladly received and they found him entertaining, jocular, and still a tease. In 1921 he was less accessible. By 1924 he had increased his troubles by getting at loggerheads with local authorities concerning accounts. During the last years of his existence he dwelt alone. He purchased supplies of food and other necessities through the kind voluntary services of Constable Henry Brock, a worthy representative of the Devon County Police, who refused all remuneration from him.

At about that time his health rapidly failed, for pneumonia intervened with other complications. In January 1925, after a serious attack of illness, he was removed to a nursing home, where on February 3, 1925, he died, at the age of 75 years. He was buried in the grave of his father and mother in the Cemetery of the Urban District Council, at Paignton.

Fig. 1. Bust of Claude Chappe.

IX

CLAUDE CHAPPE

The principles governing successful communication over long stretches of land or sea have their origin in primitive optical, acoustic, and mechanical methods of signalling. Hints of them are found in legends of Chinese dynasties, in songs of ancient Greece, in tales of Roman campaigns, and in accounts of the enterprises of Elizabethan, Jacobean, and Georgian sailors. Through revolutions, wars, and reformations the art of signalling by such means can be traced to the last years of the eighteenth and to the beginning of the nineteenth centuries, when it attained nearest to perfection at the hands of Claude Chappe—the first administrative telegraph engineer. Several biographies of this devotee of France have been published in his own country, but in personal detail they are all inadequate; the character and magnitude of his work, and the circumstances of the years of terror in which he lived, have almost obliterated the man himself. Upon that vast sea of trouble over which his endeavours to bring about unity by improved means of communication were as beacons to humanity, the track of his own career is little more than a ripple that loses itself in the gloom of his melancholy end. The best account of him is that written in 1893 by Ernest Jacquez, Librarian of Posts and Telegraphs in Paris. From this it appears that Claude's father, Ignace Chappe d'Auteroche—parliamentary lawyer and Directeur des Domaines of the King of France at Rouen—married on February 13, 1762, Marie-Renée de Vernay de Vert, of Brulon. They had ten children, of whom only seven survived, *i.e.*:

1. Ignace-Urbain-Jean Chappe,	born at	Leval,	November 26, 1762.
2. Claude Chappe	,,	Brulon (Sarthe),	December 25, 1763.
3. Marie-Marthe Chappe	,,	,, ,,	December 26, 1763.
4. Pierre-François Chappe	,,	,, ,,	August 11, 1765.
5. Sophie-François Chappe	,,	,, ,,	March 4, 1767.
6. René Chappe	,,	,, ,,	September 3, 1769.
7. Abraham Chappe	,,	,, ,,	May 6, 1773.

The father died in 1784. Claude was twin brother of Marie-Marthe. Of his boyhood little is known, except that he completed, at a small school at La Flèche, the studies begun by him at the Collège de Joyeuse at Rouen. He was educated for the Church, and he attained to being an Abbé Commendataire; this, however, did not necessitate his performing religious duties. He was early attracted to natural science, and he became acquainted with several physicists. One of his earliest experiments was with soap-bubbles that were filled with gas and then electrified. He found that if two such bubbles were charged oppositely, they attracted one another, and that under suitable conditions they detonated at contact. His researches were peremptorily brought to an end on November 2, 1789, when the benefice was suppressed by the Legislative Assembly. Claude then returned perplexed and miserable to the fatherless home at Brulon, where he found his four brothers also thrown out of employment. He rose to the occasion, however, and decided to attach himself to the new regime of republicanism in France. This he did with zeal and devotion.

During an interval of comparative leisure in the country in 1790, he conceived the idea of devising a system of communication that would permit his Government to transmit orders to a distance in the least possible time. He was aware that the problem was not new, and that it awaited solution. He discussed it with his brothers, who became his collaborators. As the result of his investigations during the next few years, three different systems were designed and operated. Briefly, they were:

(1) The Synchronized System, 1791.
(2) The Shutter System, 1791.
(3) The Semaphore System, 1792.

The first and second were examined by him tentatively. The third became established and was that most closely identified with his name.

System (1) depended upon the use of two clocks working synchronously at the sending and receiving stations, respectively. Each clock was provided with a rotating seconds-pointer that passed over divisions upon the face; and each division corresponded to a number. To transmit a phrase, the operator at the sending station struck a gong at the instant the pointer was passing the number to be signalled. A code was arranged for the interpretation of the numbers into words. In these experiments, the distance between stations was about 400 metres. As the system was impracticable for a line of many stations, Chappe endeavoured to replace the sound signal by an electric signal through a wire. Results were unsatisfactory, however, his insuperable difficulty at that time being the imperfect insulation of the conductor.

He failed to establish an electrical method. Accordingly he decided in favour of an optical system—depending upon the appearance and disappearance of surfaces of different colours and forms to indicate the precise moment of transit of the pointers past the numbers.

The first equipment used by him for (2) the Shutter System, consisted of two rectangular frames installed one at each station. The frames were fitted, respectively, with five shutters, each of which could be made to appear or to disappear at will. Further experiments revealed that elongated bodies are more clearly displayed than are shutters. System (3) was therefore devised, the equipment for which is indicated in Fig. 5.

Meanwhile, a code to be employed with such signalling devices was developed by Delaunay, a cousin of the Chappes. Delaunay, who had been on the French consular staff at Lisbon, adopted as a basis the code that had been in service for diplomatic correspondence. This remained in use until 1795.

The story of the trials of those three systems is woven into

the history of the French Revolution. A satisfactory demonstration that the synchronized system (1) could operate, was made on March 2, 1791, and the results were attested by responsible authorities. This was the beginning of a long series of experiments. The apparatus was often in imperfect condition, but it survived for more than a year, during which the Chappe family furnished means to cover the costs. Encouraged thereby, Claude, towards the end of 1791 went to Paris, and after many disappointments obtained authority to set up his machine at the Étoile. He was assisted by two of his brothers, and their efforts met with success. Success was short-lived, however, for one morning in September 1792 revolutionary fanatics destroyed their apparatus, believing it to be a device for communicating with King Louis XVI. who at that time was incarcerated in the Temple. This disaster was to some extent a blessing in disguise, for it led to a transformation of designs, *i.e.* to the abandonment of the synchronized system, and to the adoption of the shutter principle. Disaster now followed fast upon disaster—this mechanism also was smashed and burnt by the mob. Undaunted, the brothers pressed on; they started anew with a semaphore system that ultimately became standard in France. The account of this was presented by Claude to the Legislative Assembly on March 22, 1792. He appealed for protection of his apparatus, and for a fair and conclusive trial of it by competent investigators.

At that time the Assembly itself was even less secure than the apparatus they were asked to defend. They were replaced by a Convention. The Convention of October 15, 1792, transferred the request to the Comité d'Instruction, which also was too much occupied to deal with it. Meanwhile, the need for an efficient telegraph system had become urgent. Chappe, therefore, continued his efforts to perfect his machine and his code. On April 1, 1793, attention was directed to his work by Gilbert Romme (1750–95), who was impressed by the possibilities of the Chappe system. So effective was Romme's argument that the Convention decided to allocate 6,000 francs for investigating and establishing the system. Moreover, Citizen Lakanal was directed to investigate it. The result of his report was that

the National Convention accorded to Chappe the title of Ingénieur-Télégraphe, and an ordinance was published making it the duty of the Garde Nationale to protect the apparatus. The employees of the system were described as "stationnaires" —a name that continued until 1862.

Claude bore his title of telegraph engineer to the end of his days. This, and the satisfaction of having served his country, was his solitary recompense. He never forgot what he owed at this critical stage to the referees, especially to Lakanal. He thanked him for the help he had given, and he praised his courage in the encounter against ignorance and against the prejudices of such revolutionaries as Cambon and Monot. It is pleasant to be able to record the complete and lasting fellowship of the brothers Chappe in the midst of this strife. This fellowship, and this alone, enabled them to overcome the difficulties of constructing long lines of communication, to organize transport, to instruct and control the staff, and to face the situation resulting from war and financial chaos.

The circumstances of the time forced the Comité de Salut to realize the vital importance of establishing a good system of intercommunication. From August 1793 they made decrees, signed by such names as Couthon, Barère, Hérault, St.-Just, Thuriot, and Robespierre, for its immediate extension. Chappe was authorized to place his machines in any belfries, towers, or emplacements he might choose, and he could cut down any trees that might interfere with the line of vision. The owners of the land and of the trees were to be suitably indemnified, and general provision was to be made by the Government—at least in theory—for hastening the work. Supplies of workmen, cash, and the means of transport, however, were not forthcoming. Against all odds, the brothers laboured, and with such vigour that, by July 1794, a line was established and was actually working. It included the construction of fifteen stations between Paris and Lille.

Chappe did far more than devise a piece of mechanism and a code. He combined known ideas into a working system, and he caught the true spirit of telegraphy. He bridged the gulf between (1) The transmission of a signal whereby a preconcerted

message already written or otherwise recorded, and held for reference at the distant station, may be indicated, and (2) The transmission, if desired, of a series of elementary letters or numbers, whereby any message may be sent without previous conventions other than those relating to such letters or numbers. Leaving out of account the legendary telegraphs and the tentative achievements that preceded the work of Chappe, the first telegram sent and received by an organized system was that transmitted by him from Lille to Montmartre on August 15, 1794, acquainting the Administration in Paris that the French had retaken Quesnoy.

France at that time, invaded on all its frontiers and on all its coasts, was endeavouring to train a new army by a *levée-en masse.* Barère complimented Claude Chappe in a pronouncement to the citizens, ending with words too true:

> La récompense de cette invention pour les auteurs est dans la mention que j'en fais à cette Tribune.

On August 30, 1794, Chappe transmitted news of the recovery of Condé. This message arrived during one of the sittings of the Convention, and caused indescribable enthusiasm.

To-day the names Quesnoy and Condé—*i.e.* Condé-sur-Escault, 12 km. north-east of Valenciennes—have comparatively little significance. In 1793–94 they meant nearly everything. The wars of the French Revolution were at their height. The position of France was desperate. Her frontiers were assailed by allied forces including those of England, Holland, Prussia, Austria, Spain, the Italian States, Hanover, and Hesse. The road to Paris was open, and the French army was not yet able to break the lines of communication of the enemy. Marseilles and Lyons had revolted against the French Government. An English fleet, on August 28, 1793, under Vice-Admiral Lord Hood, held Toulon. Between France and perdition there stood but two barriers: (1) The incoherence of the allies, each having different objects in view ; (2) The singleness of purpose that Carnot had succeeded in establishing amongst all units of the French army. Ultimately there was added to these the annihilating force of numbers that the French *levée-*

en-masse brought into the field; for France was the most populous country in Europe. In 1793, Quesnoy and Condé had been captured by the allies, Mainz had surrendered to them, and Valenciennes had capitulated. In June 1794, Carnot—after diligent preparation—began his campaign of unremitting attack, especially against the Austrians. The allies failed to concentrate; at the battle of Fleurus, June 25, 1794, they fell into confusion and were scattered towards Holland and towards the Rhine. Then was it that the French under Jourdan regained the precious area that included the fortresses of Landrecies, Quesnoy, Valenciennes, and Condé. For about two months there was some suspension of active hostilities, but towards the end of 1794 frost set in with exceptional vigour, the low country became a continuous sheet of ice, and the French under Pichegrue and Jourdan proceeded to invade Holland. The allies fell back, the French pursued, and—welcomed by the Republican party of Holland—entered Amsterdam on January 18, 1795. A Dutch Revolutionary Committee then came into being, and some citizens of Amsterdam placed the three-coloured cockade in their hats. De Winter—who in 1794–95 was a General of Brigade in the French service—to crown the victory, moved his troops across the ice and took possession of the Dutch fleet that he was destined ultimately to command. The actual operation was led by Moreau, of the French cavalry, who with the support of a single battery of horse artillery made a dash to the north, as far as the frozen Helderstroom. Sword in hand those troops traversed the Island of Texel and, to the amazement of Europe, captured the vessels. Without the telegraph these results could not have been achieved. The French war plans were drawn up under Carnot, a Member of the Committee of Public Safety, and one of the best engineers in France. Edward Baines, writing in 1817, says:

> The balloon—hitherto considered as a philosophical toy incapable of affording any solid advantage to mankind—was converted into an elevated observatory, by means of which the position, evolutions, and numbers of the enemy could be readily ascertained; at the same time that the telegraph, with a few simple motions, served to communicate the result of a siege, or of a battle,

with the accuracy, if not the minuteness, of a dispatch, and with a celerity that in some measure rivalled the progress of sound.

It is therefore not surprising that when news of victories reached Paris, the Assembly accorded praise to Claude Chappe.

In September 1794, the Comité de Salut formulated a project to establish a telegraph towards the North—by a semaphore system on the Chappe model, associated with distinguishing signals by flags. The work was begun, but financial difficulties impeded all operations. Promises of Government assistance vanished into thin air, and before the next year was ended, employees on that line to the North were dying of hunger. Yet, at the end of that year, another line was projected. This was to connect Paris and Landau (49° N. 6° E.). The construction was to be carried out in four divisions, centred respectively upon Paris, Châlons, Metz, and Strasbourg, under the management of Claude Chappe. His duties were administrative as well as technical, and he had charge of the factory in Paris where the equipment was made and adjusted. It was at this time that he left his quarters at No. 23, Quai d'Orsay, at the corner of the Rue du Bac, and went to No. 9, Rue de l'Université—the headquarters for his new Administration.

The scheme was laid down by the authorities with extreme care, but without funds. As Engineer-in-Chief, Claude was to receive 600 livres a month, and his brothers 500 livres each a month. Wages were fixed for the work, down to those of the office boy, who was assigned 125 livres a month. Unfortunately, along the slippery path of good intentions, payments failed to keep pace with promises.

It was at that time estimated that when the telegraphs were complete it would be possible to transmit a message to the extremities of France within an hour. The prime need was to ensure service between Paris and Strasbourg. From 1794 to 1798 service was actually maintained. The line passed through fifty stations, by way of Chalôns, Verdun, Condrecourt, Saint-Quentin, Metz, Marimont and Dingsheim. Difficulties resulting from lack of remuneration of staff led to painful results. An attempt was made to pay in kind instead of in cash, but not even this materialized. In default of payment, construction

work in 1796 became disorganized. Workmen lost all zeal, and even the redoubtable Claude became despondent. Still more distressing were the conditions that he had to meet in the enterprise to the North. In his extremity, well knowing the penetrative and stimulating forces of a telegram, he turned his invention upon the authorities by dispatching to them the remonstrance:

> Des fonds, des fonds, encore une fois, des fonds, autrement nous ne pouvons rien faire. Adressez-les à Port-Malo. . . .
>
> Point encore de fonds, nous perdons depuis près de huit jours un temps extrêmement précieux . . . cette situation me désespère . . . Je fais tout pour assurer le prompt succès de l'établissement que je dirige: *de l'argent, ou point de ligne de Brest*. Salut et Fraternité. Chappe, ingénieur.

Yet he was invincible: he finished the line in seven months. It passed from Paris to Broué, Chaumont, Bruyères-sur-Bois, Avranches, Saint-Malo, Ianrodec and Guipavas, to Brest. This telegraph system remained under the administration of the French Marine until 1801. In 1799 the Strasbourg line with fourteen stations was established in Alsace for purely military purposes. It was abandoned in 1815.

An impression of Claude Chappe, loyal amidst all the turmoil, has been recorded by Jacques. It is from a secret report from a Justice of the Peace at Lille to the Executive of the Central Bureau at Paris:

> Le plus petit, qui est l'aîné et que l'on connaît ici sous le nom de l'ingénieur, parce qu'il est l'inventeur de la machine, a paru toujours joindre à beaucoup de lumières un attachement sincère à la République.

In an attempt to ameliorate conditions arising from lack of financial resources, Claude proposed to the Ministry that they should make a charge for telegrams:

For industrial affairs, commerce and banking.
For exploiting a newspaper.
For operations of a national lottery.

Only one of these suggestions was quickly put into effect—

the lottery proposal, and this—as he predicted it would—paid for the greater part of the telegraph service. Amongst his other ideas, Claude devised a system of secret communication between Calais and Dover—a project taken up by his youngest brother, Abraham. It formed part of Napoleon's scheme to invade England in 1803.

During the years 1794 to 1799 Claude appealed to all the sentiments of the Administration, he had recourse to the most ingenious means to prevent the telegraph from suffering in the general distress, he formulated measures for placing the whole upon a sound financial basis, but the combined circumstances of strife within France, and agression from without, at last weighed him down. Nerve and mind, under the load, began to revolt. The load increased, for in 1804 Napoleon demanded the immediate establishment of telegraph service between Paris and Milan by way of Lyons. Between Paris and Lyons there were ultimately fifty-eight stations.

The continual stress imposed by the new project severely tried Chappe. Nor was it only troubles arising from the work that undid him. In 1797 the usual cruel annoyances to which inventors are subjected by those who claim to have anticipated a successful device had upon him a disastrous mental effect. He took up the challenge of those who claimed priority. This action was followed by the customary exchange of letters of recrimination and revenge. Then came the end. He had gone to study, upon the terrain, emplacements for further stations on the line to Lyons. He became restless and irritable and manifested symptoms of hysteria. At the completion of the work, he returned to Paris, declaring that an attempt had been made to poison him in a village near Lyons. Finally he fell into a state of melancholy that no distraction could cure. On January 23, 1805, in a garden, his friends found his body at the bottom of a well.

He was buried first in the cemetery at Vaugirard, but subsequently at Père Lachaise, at the side of his brother Ignace, who died on January 25, 1829. The transfer took place on January 29, 1829. Their tomb bears the simple inscription "Chappe". According to M. Palhols, who investigated the

matter, the tombstone from Vaugirard was presented in 1859 to the Administration of Posts and Telegraphs in Paris. It was then cut into two slabs (Fig. 2) of half-thickness and placed at the entrance of No. 103, Rue Grenelle, which since 1840 had

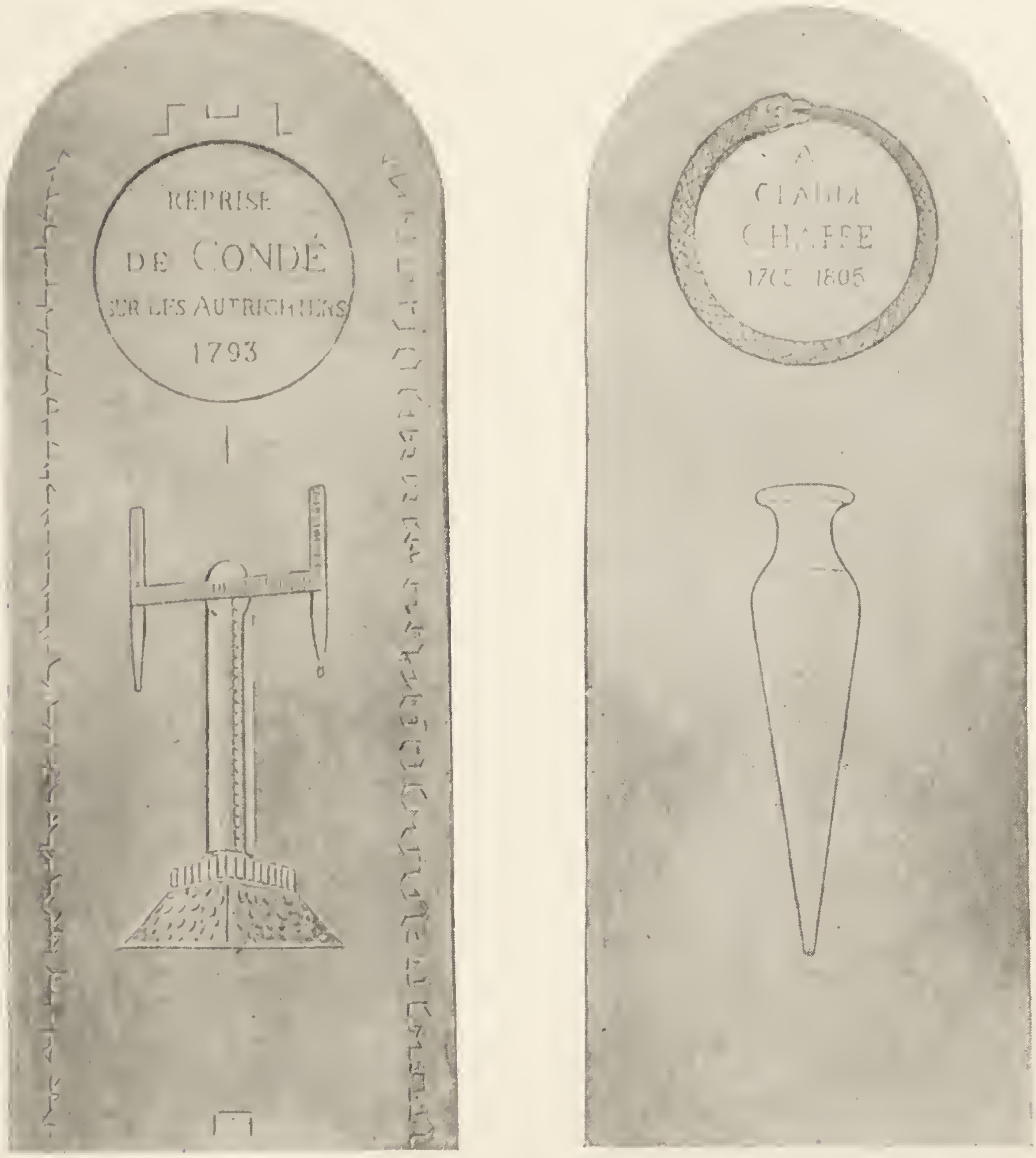


FIG. 2. MONUMENTAL STONES AT THE ENTRANCE TO THE ADMINISTRATION OF TELEGRAPHS IN PARIS, NO. 103, RUE GRENELLE.

been the headquarters of that Administration. The inscription contains two errors, for Claude Chappe was born in 1763, not in 1765; moreover, Condé was recovered in 1794, not in 1793, the date upon the stone.

It has been remarked by his biographers that the Chappe telegraph became famous first by the announcement of the

recovery of Quesnoy in 1794, and that it ended its career in 1855 when it was called upon to transmit an account of an incident of like consequence—the taking of Sebastopol. In the Crimea, before transmitting this last message, it had served for eighteen months the military interests of the French army, and it had throughout moved with the troops.

Amongst the last of the Chappe telegraph services may be recorded:

> Narbonne to Avignon by way of Montpellier (1831–34).
> Avranches to Cherbourg (1833).
> Avranches to Nantes.
> Bordeaux to Narbonne by way of Toulouse (1834).
> Narbonne to Perpignan (1840).
> Dijon to Besançon (1840).
> Bayonne to Behobie (1846).

In France, the Chappe telegraph was for many years in service, in harbours for signalling to vessels, on battlefields for signalling the positions and movements of the enemy, in sieges for signalling to the relieving contingents. By its means the Minister of War corresponded with Army divisions, and the Minister of the Interior with the Departments of France. At Paris, when this system had been perfected, they could receive communications from Lille in two minutes, from Calais in four minutes five seconds, from Strasbourg in five minutes fifty-two seconds, from Toulon in thirteen minutes fifty seconds, from Bayonne in fourteen minutes, and from Brest in six minutes fifty seconds.

From Paris to Behobie, at the far south-west of France in the Basses-Pyrénées, a signal could be sent under normal circumstances in about forty minutes. By 1852, at the moment that the electric telegraph was substituted for the Chappe telegraph, the Chappe lines comprised nineteen branches, and a total length of more than 4000 kilometres, including 556 stations. Similar lines, of a military character, working on a simplified system were established also in Algeria.

The angular settings of the wings were limited to seven positions, each 45° from the next—in practice making sixty-three variations, or 262 when combined with the four movements

of the cross-arm. In its later form each setting could indicate a syllable or an entire word. The range was limited to that of their telescopes—*i.e.* to from six to eight miles on the level. The nearest telegraph to Paris was then at Montmartre. From

Fig. 3. Portrait of Claude Chappe.

Paris to Lille, from ten to twelve stations were necessary. At night, torches or lanterns were sometimes used upon the arms of the machine. In another method, for night service, translucent illuminated code signals, or figures, were employed.

In 1795 Chappe had pointed out that benefits might be conferred upon France by transmitting weather reports by his

telegraph. A weather report service, however, was not completely organized until 1856.

The word "télégraphe" does not appear in his communication to the Assemblée Législative of March 22, 1792. On that occasion he described his invention as a "tachygraphe"—rapid writer. In April 1793 the word was changed to "télégraphe"—the far writer—as the result of a conversation between Ignace Chappe and Miot, Chef de Division à l'Intérieur.

Chappe was fully aware of the extent to which, in ancient methods of communication, the general principles of his device had been adumbrated; he knew that the value of his work was in the adaptation of those principles to the needs of his time. He designed the mechanism to secure the greatest visibility, strength, lightness, durability, and ease in operation.

The alternative systems (1) (2) and (3) happen to have been investigated by him in the sequence in which the basic principles came down through the ages. The first synchronized method in Europe probably dates from about 300 B.C., for the Greeks adapted their clepsydrae to the purposes of signalling. The clepsydrae in effect were water-clocks depending upon a steady flow of water—literally the "*stealing*" of water—from or into a vessel containing a float. The float carried a vertical rod or flat strip. Upon such a strip, phrases could be written in horizontal lines, or the letters of the Greek alphabet could be displayed in a vertical line. Arrangements were made for similar equipment to be supplied at successive stations along the line of communication. By raising a torch, the flow of water could be ordered to start at each station simultaneously. By raising the torch a second time, the flow could be stopped at all stations, and the message could be read opposite a datum mark on the apparatus. In addition, the Greeks made use of a method sufficiently explained by Fig. 4. The plan of communicating letter by letter was abandoned, and did not enter again into practice until the sixteenth century. The Romans and Gauls made use of signalling towers on a vast scale, and they certainly had a system of prearranged phrases with identification signals by beacon fires. Frequent use was also made of a line of signallers who shouted from mouth to mouth. Homer states

Fig. 4. Ancient Greek method of signalling by Torches. The alphabet is divided into five columns, of which four have five letters and one has four letters. Torches are hidden behind two walls—one to the left and the other to the right. To indicate the twenty-fourth letter, expose five torches to the right, which indicates the fifth column, and four torches on the left to indicate the fourth letter in that column. Tubes were fixed on each wall to direct the view. This method produced only feeble results.

that the voice of Stentor, the Greek herald in the Trojan war, was as loud as that of fifty men combined. Alexander the Great (356–323 B.C.) is said to have had means whereby a stentorian voice could be heard by all his army.

Questions of priority concerning pre-electric telegraphy are rendered complex partly because of the slow development of ancient devices, and partly because that development related not only to equipment, but to codes, cyphers, vocabularies, dictionaries, alphabets, and numerical arrangements, all directed towards facility of transmission. Questions of priority relating to early forms of telegraphy were disposed of once for all by Bouvat, a missionary to Asia, who pleasantly declared that the Chinese had used the methods for 4000 years, and that they were all invented by Fohi, the founder of Chinese science.

Following upon the earliest workers, groups of inventors, including Kirchner (1550), Scheventer (1636), and the Bernouillis, endeavoured to transmit messages by means of musical instruments, each note representing a letter. One of the Bernouillis devised an instrument formed of five bells, whereby he could express all the elements of the alphabet.

The method of transmission of signals in England during the middle ages consisted in hoisting barrels or beacons upon masts, towers, or hills. At the beginning of the sixteenth century, occult science was in vogue, and those possessed of rudimentary scientific knowledge often preyed upon credulity by suggesting that they had powers of communicating by mirrors through the agency of the moon and stars and magnetic influence. François Kesler signalled letters of the alphabet by means of a lamp suspended from a hook in a subterranean tunnel. In front of the lamp was a screen that could be raised or lowered by a lever. He also signalled by the aid of smoke issuing from a barrel. In Kirchner's method, described in his *Ars magna lucis umbrae,* the message was written upon a mirror, the sun was the source of light, and the light was converged upon the distant station by a lens. It was a kind of magic lantern. Kirchner's object was "not merely to communicate the most secret thoughts of the heart to a distance, but also to transport to the eyes of a friend at an enormous distance

your profile or silhouette". Thus Kirchner anticipated television. In 1663, the Marquis of Worcester suggested a day or night plan of signalling by exposing letter shapes.

There is a legend that in 1670 the King of England heard speech shouted through a trumpet from a distance of $1\frac{1}{2}$ English miles, and that he sent the trumpet to Deal Castle. The Governor of Deal Castle reported that the trumpet enabled communication to be carried on across three English miles of sea. It was invented by that "ingenious mechanist" of Hammersmith, Sir Samuel Morland (1625–95). The mouthpiece was shaped to prevent lateral loss of sound. Morland wrote a treatise on this *Tuba Stentorophonica*, and in 1666 he also gave an account of "A new method of Criptography".

In 1762 Benjamin Franklin experimented with the transmission of sound under water. In 1783 came Ganty and Biot with their tubes. They transmitted speech through 395 metres of tube by speaking very loudly. At 951 metres, speech was scarcely audible.

In 1763 Monsieur de Morogues published his *Traité des Évolutions et des Signaux*—a theoretical treatise on signals in which he introduced the idea of having a special flag to indicate the signification of a set of flags, for example, numbers or letters.

A noteworthy advance in optical communication was made when so-called "Indian Fire" was imported to England for signalling purposes. It was used with specially good effect in 1787, when by means of it the Observatories of Paris and Greenwich were for the first time brought into agreement, across the English Channel. The "Indian Fire" was contained in small boxes that burnt for about $2\frac{3}{4}$ minutes; it is said to have been unaffected by wind or rain.

Under the title of "Synthematographie", F. A. B. Bergstraesser wrote, in 1785, a treatise in German on the earlier modes of signalling. He described methods of communication that had been used by day and by night for military and other purposes during the centuries. It was estimated that one of the schemes would require 6000 or 7000 "coups de canon" to dispatch twenty words fifty miles. He explained certain numerical

and alphabetical codes, and the employment of various shapes and solid letters for mechanical signalling. Moreover, he gave an account of methods of transmission by rods and bevel gears, shutters, pivoted arms set at various angles, torches, lanterns, and in fact, by the whole of the devices available up to, and during his time. A copy of his work, in five volumes, was presented by him to George III., and is now preserved in the British Museum Library.

Another remarkable treatise entitled "Histoire de la Télégraphie" was written by Claude's elder brother, Ignace Urbain Jean Chappe, who is described as "Ancien Administrateur de la Télégraphie". This was published in Paris in 1824. A second edition appeared in 1840. It embodies some of what is told by Bergstraesser, but in addition, it gives an account of the scientific principles to be observed, especially with regard to visual signalling: it reviews the contemporary state of knowledge of the subject throughout Europe at the beginning of the nineteenth century, and it includes some precious details concerning the work of Claude. This book contains a valuable introduction by Abraham Chappe, the youngest brother of Claude, in which is reviewed the history of the subject, with comments upon the part taken by the Chappe family in its development. Figs. 4-8 are reproduced from this treatise.

Between 1808 and 1819 indirect light was thrown upon the subject by John Macdonald, F.R.S., a retired Lieutenant-Colonel of the Royal Artillery. In one of his contributions, he purported to describe "Experiments upon the Relative Times of Burning of Fuses"; but, fortunately for telegraph history, he digressed widely to attack "the present imperfect state of telegraphic communication". As early as 1797, Macdonald in England emphasized the importance of developing telegraphy as a science; he also prepared a telegraphic dictionary, and a three-figure system for conveying words, phrases, and sentences. In 1810 he laid his scheme before the naval authorities. Mr. Barrow—who was then "the man of science at the Admiralty"—declared the dictionary to be "precisely what is wanted". The result was, to some extent, satisfactory for the Royal Navy. Spelling was avoided. Everything was by numbers.

Macdonald's next step was to point out that the telegraph, as it was on land in 1810, merely expressed one figure at a time. He urged the Admiralty to adopt a three-figure system for their line telegraphs.

He says that shutters were judged to be better adapted than semaphore arms to the English climate "it being supposed that a certain number of shutters would be better seen than the same number of arms acting conjointly". This judgment, however, is in direct opposition to that of the Chappes. The shutter telegraph to which Macdonald refers gave sixty-three combinations. In the Royal Navy, at that time, three and sometimes four figures were telegraphed simultaneously, *i.e.* by one hoist of flags. He directed attention also to the relative merits, in regard to visibility, of a shutter and a "semaphoric wing". In particular, he pointed out the difference in this respect between an arm 9 feet in length lifted high in the atmosphere, and a shutter grouped with five others at a low station. He asked for a trial of shutters against arms on a line of eleven or twelve miles. His book contains the following remarkable peroration:

> It is unnecessary to dwell here on the incalculable benefit that would arise to commerce, to public revenue, to private convenience, and to public safety and security, by establishing a ramified telegraphic system, extending from the metropolis to the principal seaport towns, inclusive of a methodized intercourse with the principal cities situated to the right and left of such lines of communication. Such an undertaking would be a sublime attempt at an approximation of time and space; and would be truly worthy of the high character of our mighty nation. I, an obscure and humble individual, advanced in age, and passing on to that country from whose bourne no traveller returns, venture to prophesy that future ages will see this magnificent idea fully realized. Let it be recollected that a less simple plan, the establishment of mail coaches, was at the time deemed impracticable and visionary. Man is a progressive animal.

Macdonald devoted great attention to the question of a numerical system. In the development of telegraphy, this was an important step, and he sums up the situation in a paragraph as follows:

The most essential improvement in naval signals has arisen from the invention and application of *numerical order*. This simple, but luminous improvement is generally ascribed to Monsieur de la Bourdonnais; but those who have looked closer into the subject, know that Bishop Wilkins in his *Secret and Swift Messenger* not only recommended the method of signalling by notation, but describes the mode of execution. Dr. Hook, who is the inventor of the Land Telegraph, a species of which he mentions, recommended a numerical plan to the Royal Society. Kirchner very nearly hit on the invention, and Gaspar Shottus in his *Technica Curiosa*, expressly mentions it. With all this, it must be confessed that Monsieur de la Bourdonnais brought the plan to considerable perfection.

The book referred to is *Mercury, or the Secret and Swift Messenger, showing how a Man may with Privacy and Speed communicate his Thoughts to a Friend.* It is by John Wilkins (1641), and is an ingenious work on cryptology and modes of rapid correspondence.

Between the years 1784 and 1788 a method of signalling by means of troops formed up into various shapes was suggested; and a Dutch Officer, Boucheuraeder trained soldiers to perform in this manner in Holland. Telegraphy became in fact, a vogue, and there were not lacking then as now telegraphomaniacs who pestered Administrations with impracticable schemes.

Bergstraesser, Macdonald, and Ignace Chappe, all paid tribute to a much earlier writer, Sir Robert Hook (1635–1703) the mathematician and advocate of aeronautics. The discourse given by this great Englishman at the Royal Society of London on May 21, 1684, is concise and instructive:

Dr. Hook's Discourse to the Royal Society, May 21, 1684, *on "showing a Way how to Communicate one's Mind at great Distances."*

That which I now propound, is what I have some Years since discoursed of; but being then laid by, the great siege of Vienna, the last year, by the Turks, did again revive in my Memory; and that was a Method of discoursing at a Distance, not by sound, but by Sight. I say, therefore, 'tis possible to convey Intelligence from any one high and eminent Place, to any other that lies in sight of it, tho' 30 or 40 Miles distant in as short a Time almost, as a Man can write what he would have sent, and as suddenly to receive an

answer, as he that receives it hath a Mind to return it, or can write it down on Paper. Nay, by the Help of three, four, or more of such eminent Places visible to each other, lying next it in a straight line, 'tis possible to convey Intelligence, almost in a Moment, to twice, thrice, or more Times that Distance, with as great a Certainty, as by Writing.

For the Performance of this, we must be beholden to a late Invention, which we do not find any of the Antients knew; that is, the Eye must be assisted with Telescopes, of Lengths appropriated to the respective Distances, that whatever Characters are exposed at one station, may be made plain and distinguishable at the other that respect it.

First. For the Stations; if they be far distant, it will be necessary that they should be high, and lie exposed to the Sky, that there be no higher Hill, or Part of the Earth beyond them, that may hinder the Distinctness of the Characters which are to appear dark, the Sky beyond them appearing white: By which Means also, the thick and vaporous Air, near the Ground will be passed over and avoided: for it many Times happens, that the tops of Hills are very clear and conspicuous to each other, when as the whole interjacent Vale, or Country, lies drowned in a Fog. Next, because a much greater Distance and Space of Ground becomes visible, insomuch that I have been informed by such, who have been at the Top of some very high Mountains, as particularly at the Top of the Pike of Teneriff that the Island of the Grand Canaries, which lies above 60 Miles distant, appears so clear, as if it were hard by; and I myself have often taken Notice of the great Difference there is between the appearing Distance of Objects seen from the Tops and Bottoms of pretty high Hills, the same objects from the Top appearing nearer and clearer by half, and more than they do when viewed from lower Stations of the Hills; and this not only when the Space between them was land, but where it was nothing but Sea. I have taken Notice also of the same Difference from the Prospect of Places from the top of the Column at Fish-street-Hill, where the Eye is, in good Part, raised above the smoky Air below.

Next, the height of the Stations is advantageous, upon the account of the Refraction or Inflections of the Air; which Inflections of the Air are many and very great, sometimes in an Air which seems, to the naked Eye, the most clear and serene. Insomuch that That alone does wholly confound the Distinctness of Objects appearing at a Distance; now the greater Part of these arise from Commotions of the more dense Air that is near the surface of the Earth, by the Rarefaction of some Parts of it, caused by Heat; which rarified Parts ascending, do make the Objects seen

through it, to seem to dance and undulate, which is in great part avoided, if the Prospect be from an higher Place. Besides, the Nature of the Air itself, at great Heights, approaches nearer to the Nature of the ether, which more powerfully propagates the Impulses of Light.

He then explains that there must be no hill interposed between stations, because the air above that hill will be very apt to disturb the clear appearance of the object. There is to be one telescope at each terminal station, and two at each intermediate station, and times are to be agreed upon for operation.

Next, there must be a convenient Apparatus of Characters, whereby to communicate any Thing with great Ease, Distinctness, and Secrecy. There must be therefore, at least, as many distinct Characters, as there are necessary Letters in the Alphabet that is made use of . . . and those must be either Day Characters or Night Characters.

His plan was to use shaped letters by day and torches by night, to represent either letters or whole sentences. He adds:

I could instance a hundred Ways of facilitating the Method of performing this Design with the more Dexterity and Quickness and with little Charge; but that, I think, will be needless at present. . . . The same Character may be seen in Paris, within a Minute after it hath been exposed in London.

He realized that there might be many applications of such a method, particularly for cities or towns besieged, and for "Ships upon the Sea".

Sir Robert Hook utilized two or more shapes to form a multiplicity of signals, as probably the Greeks did in time immemorial. Ignace Chappe points out that in like manner Sébastian Truchet had combined two tiles—each coloured in two tints arranged diagonally—to give sixty-four changes. Here was the germ of the *dot* and *dash*, and of kindred two-tone and double-key systems of telegraphy. Ignace Chappe gives credit to Buria, a member of the Academy of Science of Berlin, for having suggested the reduction of the alphabet to a set of signs formed of two characters, and for pointing out that by such a binary system, for example, two prisoners might communicate by two different sounds—a knock and a scrape—which could

be done by the heel of a boot, another precursor of a two-tone scheme.

From this sketch of the condition of knowledge concerning telegraphy in the early days of the Chappes, it is seen that the task before them was not to discover but to select and to co-ordinate. Of ideas there was untold wealth; of those powers of discrimination that spring from scientific research there was lamentable poverty. The importance of scientific investigation is well illustrated by the advance made by Claude consequent upon his mastery of the principles of visibility. Ignace has left a record of the results of these investigations. He explains that although it is easy to signal by an arm or a rod for a short distance slowly, difficulties increase rapidly with the distance and with multiplicity of stations. The moving part, whatever its shape, must be light enough for transport up mountains, towers, and other buildings; it must have surface enough to be seen, and yet strength enough to resist wind. It must therefore be exceptionally strong. Movements must be rapid and simultaneous, they must demand very little force, they must be capable of exact repetition without confusion, they must be free from ambiguity, they must remain unaltered during exposure. The brothers found it well to have available a large number of primitive signals for service matters, and a phrase-code for the operator. Economy of time had always to be considered. Exceptional difficulties arose, as Hook had predicted they would, from atmospheric changes and from the effects of refraction of heated air. Special attention had therefore to be given to the form and colour of the exposed bodies. Experiments proved that upon a white ground a black disc is lost sight of at less distance than is a black line of the same width. Of two unequal black lines of the same breadth, the longer is visible from a point more remote than is the shorter. Black is preferable to white, because the visibility of the contours of an opaque body results largely from contrasts between the object and the background. There is always danger that an exposed body will be confused with what is around it, even if it is a white body upon a black background, because the contrast only relates to the points or lines of apparent contact between two surfaces.

Contrasts are diminished by vapours, clouds, and refractions. Black backgrounds diminish contrasts. To avoid these difficulties they raised the bodies above all other terrestrial bodies —as modern "sky signs" are raised.

Experience showed that the shape of bodies seen at a great distance is lost when they reflect directly the light of the sun. It was for this reason that Chappe used planes inclined in various directions to form contrasts, or as Ignace says: "faire contraster, par ce moyen, le télégraphe avec la diaphanéité" of the atmosphere. The arms were therefore formed of inclined planes, resembling the sails of a windmill (Fig. 5).

Experiments were made to determine the best shape of opaque bodies for the purpose. A long parallelogram was found to be best. It has to be of comparatively slender construction, so as to present small resistance to wind. The battens were spaced to allow free passage of air, and they were sloped to prevent direct reflection of the sun's rays.

At first, the diametral arm (Fig. 5) was arranged to stop in one or other of six angular positions. Ignace says, however, that a body that is not perceived when it stands alone may become so when it is near to another. Consequently, the number of angular positions assigned to the diametral arm was reduced to four, and a wing was added at each end of it. These wings were set in angular positions, thus increasing the number of signals that could be made with the machine. Each of the two wings could take seven positions with regard to the arm, the result being that $7 \times 7 \times 4$ shapes were in general obtained.

Another principle at which they arrived was that acceleration is best achieved, not by unduly quickening the movements, but by care in ensuring that each signal conveys a clear idea. They therefore concentrated upon devising a good signal-manual and upon obtaining perfect control of the operators so as to avoid faults. Beyond a certain rate, quickening the movements was ineffective, because—as nearly simultaneous signalling was carried out at all the stations on a given line—there was overlap between stations in point of time. The essentials to be attended to therefore were perfection of code and faultless operation. The elimination of faults was dealt with in a drastic

manner. Although the revolutionaries sang of liberty, equality, and brotherhood, the quality of mercy was not in their composition—negligence was punished by imprisonment. The mem-

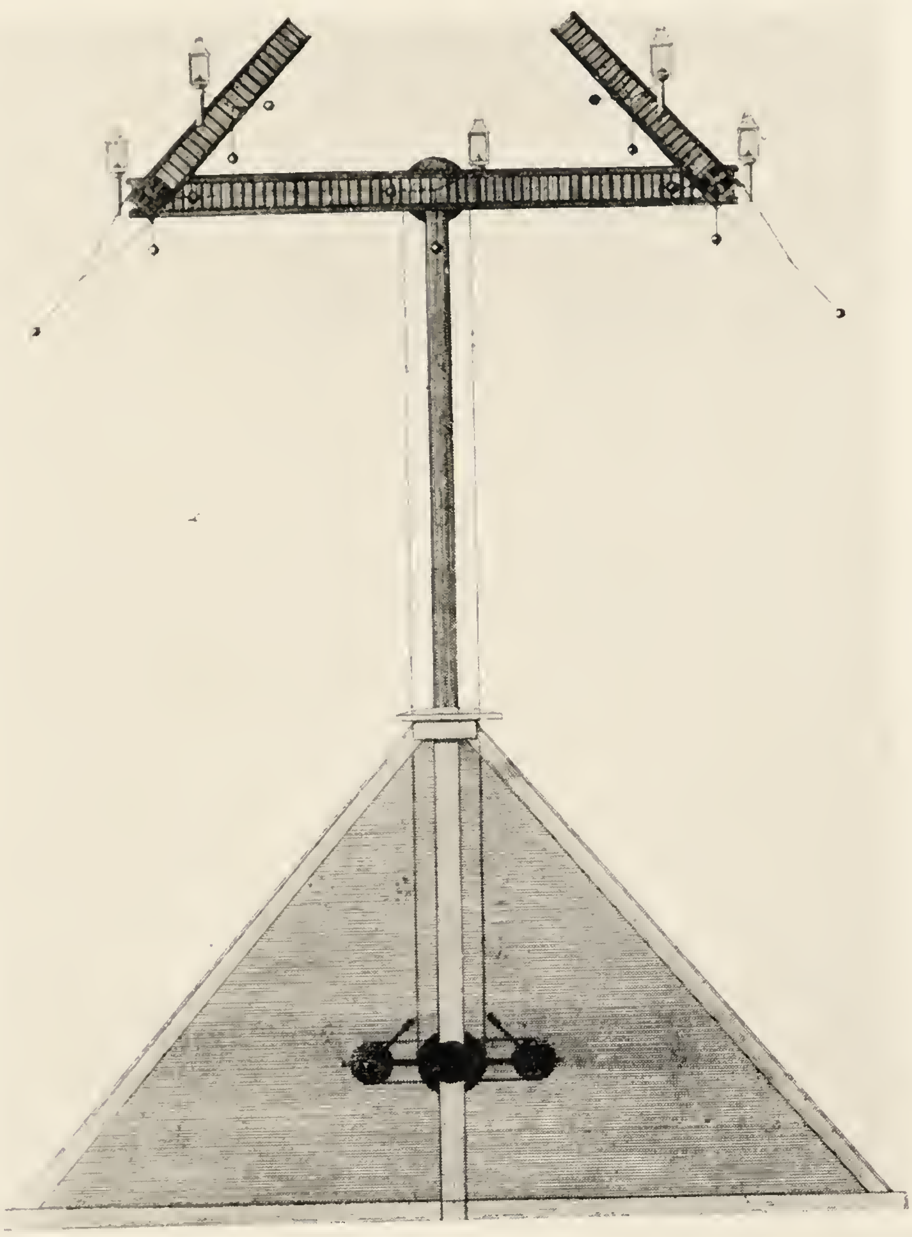


FIG. 5. "LE TÉLÉGRAPHE FRANÇOIS", as designed by Claude Chappe. It consists, at its upper part, of three pieces, each of which is able to move independently. The large centre-piece or main arm is a long parallelogram, at the extremities of which are fitted the other two pieces or wings, each able to take four angular positions 45° apart. Together they can produce 196 shapes, each of which has a conventional value. The small "répétiteur" below the structure can be observed by the operator to check the movements.

bers of the staff at each station were placed under a chief who could dismiss at will. To secure correct transmission, operators were selected of dull intellect, without ideas and without ambition. Wages for such "stationnaires", when wages were available, were twenty-five sous a day.

At intermediate stations the "stationnaires" had only to copy what was seen. Operators at terminal stations did all the code work. At each station there was a repeater (Fig. 5) inside the building to indicate what was being transmitted. In one system the signal was set when the arm was diagonal, and it was only read when it was either horizontal or vertical, *i.e.* in the confirmatory position. The movement from oblique to vertical or horizontal assisted the visibility of the signal.

Upon the Chappe principle, the French utilized for military purposes "télégraphes ambulants". In October 1797, the French Government appointed Bonaparte to command an expeditionary force to attack England. In February of the next year he visited the French coast near Calais, and while he was pondering on a descent by a flotilla of small craft, it was realized that it would be very helpful to have at Gris Nez the means of signalling at night to Dover, while making the English coast. Accordingly, a powerful lamp was devised to work in conjunction with a large telegraph. The light from the lamp was to be projected by means of parabolic mirrors sixteen inches in diameter. It was upon the occasion of abandoning this scheme that Bonaparte declared that to effect a landing upon England without being master of the English Channel would be the most temerarious and difficult operation ever attempted, and it was for this reason that he diverted his attention to Egypt.

The Chappe system was adopted in Egypt with considerable success, because it was not subjected to alterations as in other countries. It is recorded that messages were sent between Alexandria and Cairo in forty minutes.

In 1794 Endelerantz developed the Swedish telegraph upon a plan somewhat resembling that of Chappe. A vertical post was provided with two arms forming a cross. The arms could be set at various angles to indicate letters. The apparatus did not produce enough signs for practical purposes. Endelerantz therefore substituted another design resembling a large rotating dovecot with shutters. This was mounted upon a central vertical axis. At night he used occulting lanterns upon somewhat the same principle as the modern signalling lamp. The first trials of the Swedish telegraph took place between Drottning-

holm and Stockholm on October 30, 1794, and subsequent trials on August 30, and October 18, 1795.

When looking back upon the work of Chappe and his

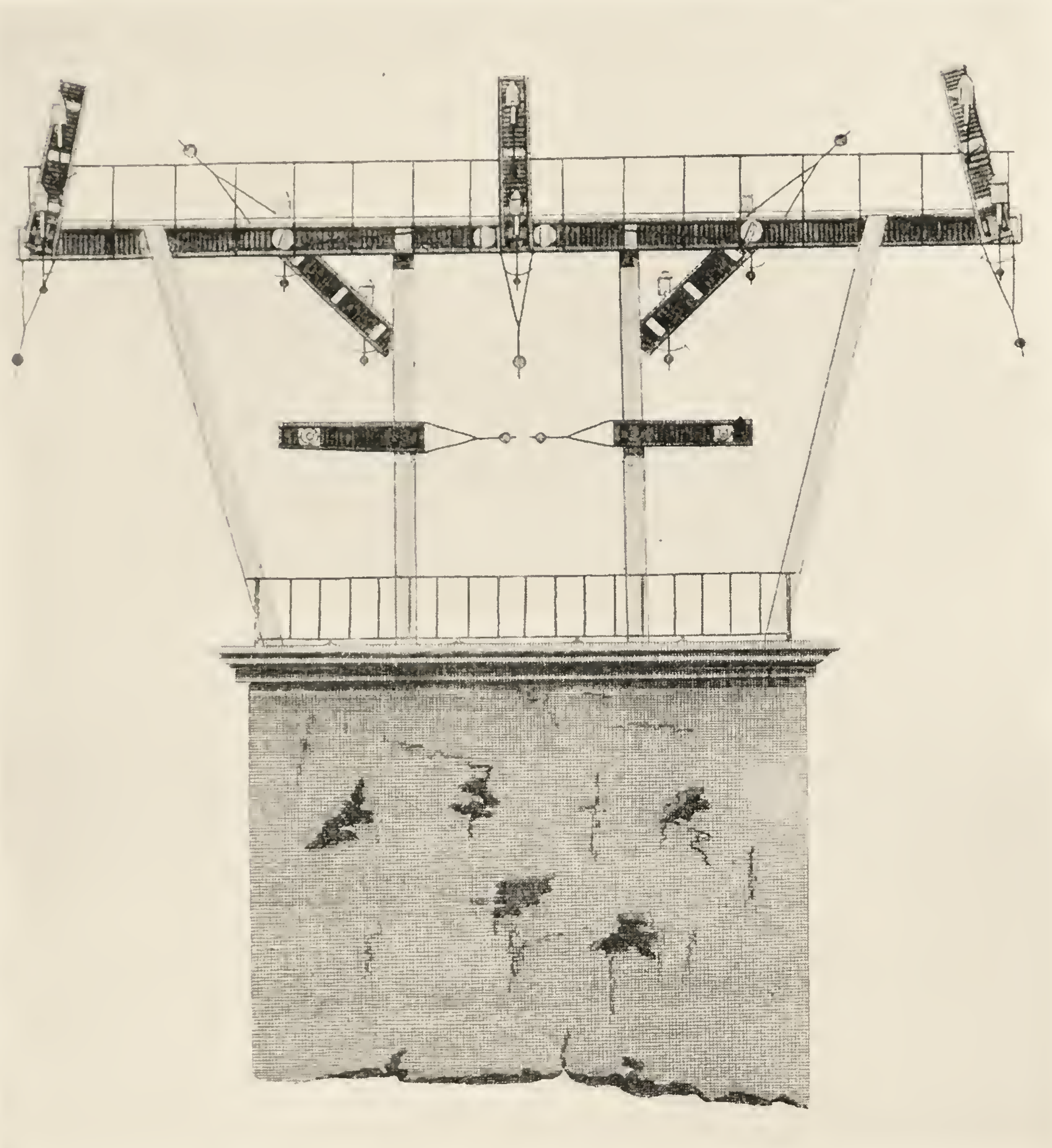


FIG. 6. THE MACHINE INVENTED BY MONGE ABOUT 1798. It was placed for a long time at the Tuileries, and a great number were constructed to operate on the line to Landau. Another machine was set up near to Metz. According to Ignace Chappe, it never came into service.

brothers, it is seen that they succeeded in solving three problems, and in uniting the results to establish a practical system of optical telegraphy. The first was the design of a machine, the second was to arrive at the principles in accordance with which the physical conditions could best be met, and the third was,

out of the chaos of ancient suggestion, to evolve a working code. It fell to the Chappes, in addition, to face administrative, political, and military difficulties, and to succeed in circumstances of exceptional privation and danger. From beginning to end they were sustained by loyalty to France; their names deserve to be remembered as engineers who devoted themselves conscientiously to the welfare of their country, and as pioneers who made things easier for electrical communication that was to follow.

The development of the visual telegraph in France had its counterpart in England. A hint of the way in which information concerning the Chappe system was obtained by England is contained in Ignace Chappe's *Histoire.* Some other details of the development in England are to be found in an article by O. Tuck in *The Fighting Forces*, Vol. 1, 1924. Codes and signals are dealt with in the famous treatise *British Flags*, by W. G. Perrin, Admiralty Librarian. From these various sources it appears that during the wars with France, at the end of the eighteenth century, a British officer at Menin observed what at first he thought to be a windmill with only two remaining sails. Occasionally the sails changed their angles with the horizon and seemed to make signals which were followed by the French troops. The matter was reported to John Gamble—Chaplain to a member of the Royal Family. Gamble prepared an account entitled "Observations on Telegraphic Experiments", and in 1795 sent a copy of this to the Admiralty. He designed and constructed apparatus consisting of a vertical frame holding five shutters which could separately be opened or closed, allowing for thirty-one changes. Tuck mentions that at the time the Admiralty received Gamble's report there was no original telegraph system in England. Between the North Foreland and Land's End the Admiralty had forty-seven signalling stations. By means of hoists of various groups of black balls from yard-arms at these stations, prearranged messages were communicated.

Preceding Gamble, another "inventor", Murray, fourth son of the Duke of Atholl, had produced a device comprising six shutters that gave sixty-three changes. Gamble and Murray

were both in holy orders; and by another coincidence they both hit, in effect, upon a shutter telegraph, such as Chappe had long before abandoned in favour of a semaphore system. Gamble set up his trial shutter machine on August 6, 1795. He followed by designing a telegraph having radial arms, and

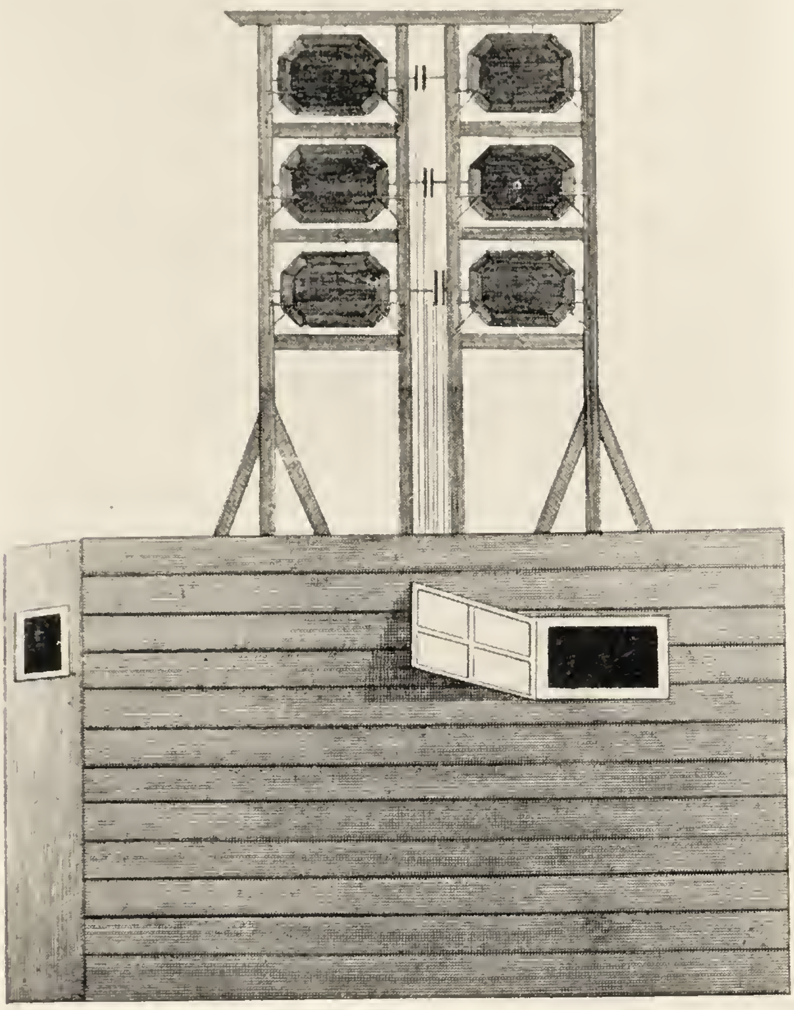


FIG. 7. APPARATUS FIXED ON THE ADMIRALTY BUILDING, WHITEHALL, LONDON, IN 1796. A frame with six shutters. It was sometimes necessary to make more than one signal to express a letter. Moreover, the proximity of the shutters to one another was apt to lead to confusion, especially in a smoky or foggy atmosphere. It is said to have served only twenty-five days in a year. A different machine, similar to Fig. 9, was substituted, probably about the year 1810.

offering this to the Admiralty. Their Lordships replied that they were

. . . so well satisfied with the Telegraphs erected under the direction of Lord George Murray that they did not think it necessary to make any experiment with the radiated form.

To soften the blow, the letter was signed "Your affectionate

friends". The truth is, the contribution of Gamble added nothing to the information already in the possession of Their Lordships.

In the same year, 1795, a surveyor—George Roebuck—contracted to erect for the Admiralty fifteen stations between London and Deal at £230 a station. An eight-guinea clock and two twelve-guinea telescopes at each station completed the equipment. By January 27, 1796, a signal could be sent from London to Deal, and acknowledged, in two minutes. Roebuck was then appointed Superintendent of Telegraphs, with a salary of £300 a year. This work was followed by a line through Putney Heath and Beacon Hill to Portsmouth, with ten stations including Chelsea and the Admiralty. There was also a project in 1801 for a line to Yarmouth, but the peace of Amiens intervened and the scheme slumbered. So profound was the peace that signal stations along the eastern and southern coasts of England were abandoned to the owners of the land on which they stood.

War in 1803 again shook the world, and in 1805 Roebuck was active between London, Portsmouth, and Plymouth. The Plymouth line—200 miles—was completed on July 4, 1806, and Tuck says that a reply from Plymouth began to be spelt out only twenty minutes from the time that same telegraph had made "Message ended". The 1 o'clock signal could be made and acknowledged in three minutes.

In 1807 the Yarmouth scheme was resuscitated—the trail was through Chelsea, Hampstead, St. Albans, and the Gog Magog Hills near Cambridge, in all, eighteen stations. By the middle of 1808, Roebuck had altogether sixty-five stations under his supervision, operated by the shutter system.

Between the years 1811 and 1816 the semaphore gradually replaced the shutter system in England. With the peace of May 1, 1814, when Napoleon was banished to Elba, there was again general relaxation and abandonment of signalling stations. But, fortunately for English telegraphy, he escaped and landed in France on May 1, 1815. Telegraphs were re-established with all haste and there was a general order to adopt the semaphore method. In November 1815, a line was

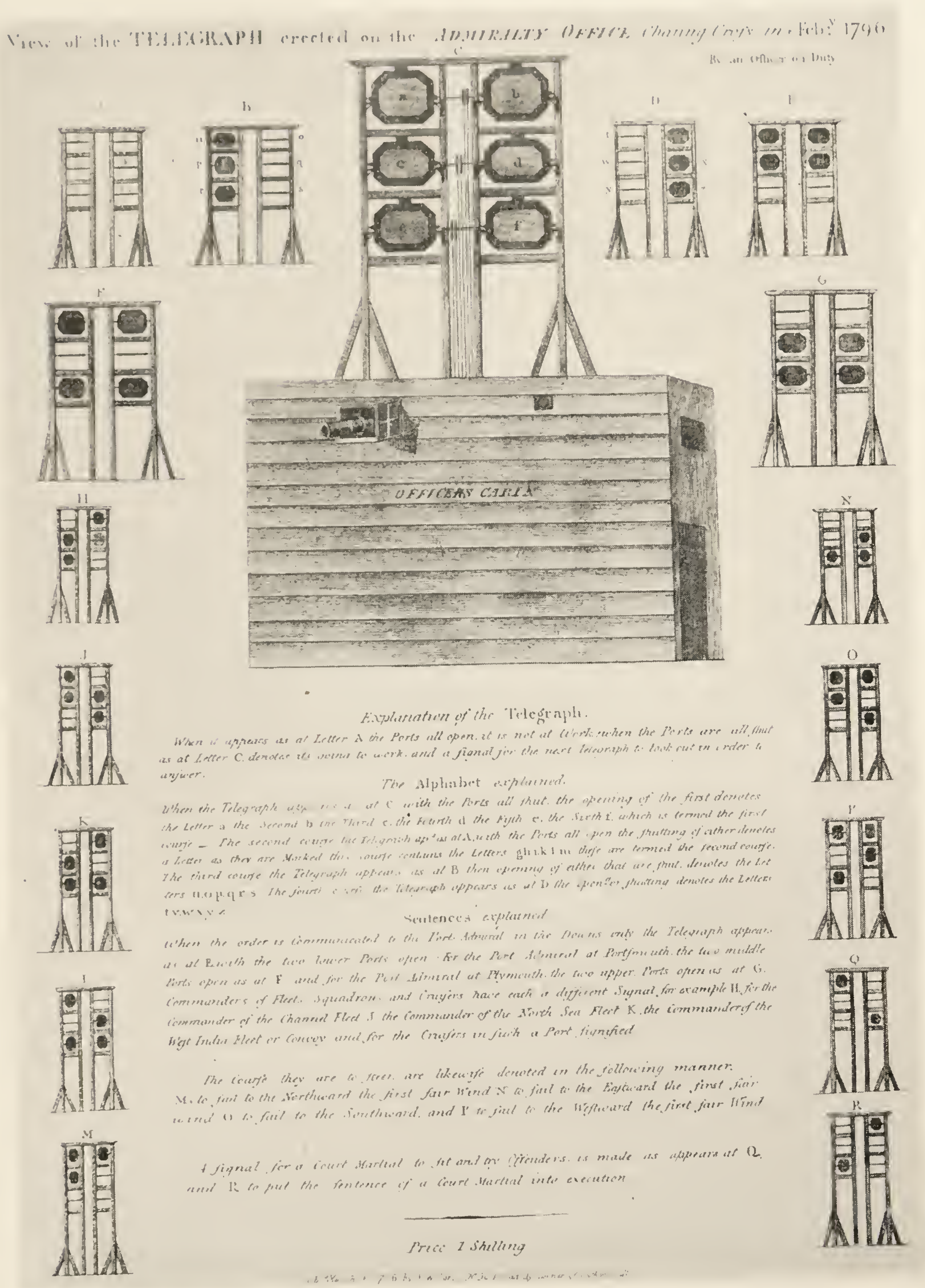


FIG. 8. COPY OF A PRINT, now in the Admiralty Library, of the Telegraph of 1796, with explanation of the Telegraph. Reproduced by kind permission of the Admiralty Librarian.

projected through West Square, Southwark, Nunhead, and Red Hill, to Chatham. Peace, however, followed Waterloo, the telegraphs were all paid off, and Roebuck's work was at an end. The Chatham line had nevertheless established the superiority of the semaphore method. In 1818 there was an effort to

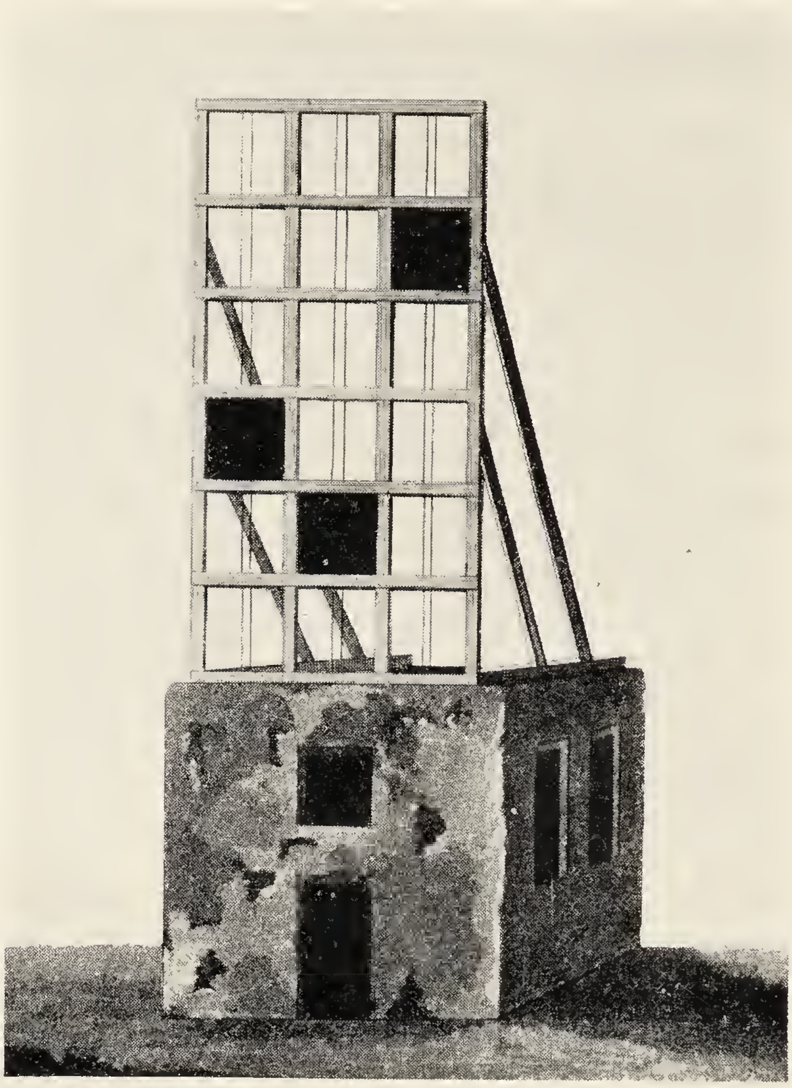


FIG. 9. APPARATUS FIXED AT PLYMOUTH. It had only three shutters. These did not turn upon an axis, but they could be made to slide up or down to take a position at any of five openings in the frame.

restore the telegraph service, especially on the line to Portsmouth.

In 1819 the English Ambassador in Paris requested the French Government to give England a model of the French telegraph. This was immediately done; but as the model was not accompanied by instructions, it was not used in England to best advantage. Although stray information concerning it had been obtained earlier, the principles necessary for its proper working had not yet been grasped in England. This

Chappe machine was installed at Nunhead, where it remained until it was replaced by the Popham machine. By 1830 the telegraph system in England was again almost out of action. In 1836 Wheatstone was transmitting electrical signals from

FIG. 10. MODEL PRESERVED IN VERKEHRSMUSEUM, NÜREMBERG, of the optical telegraph used at Möllenkopf, near Ehrenbreitstein, on the Rhine in 1833.

London to Birmingham and on December 31, 1847, the last of the English semaphore stations closed and the occupants of stations were discharged.

To link the history of the Chappe system with that appertaining to earlier methods of signalling, so far as England is concerned, requires retrospective vision. In the reign of Queen

Elizabeth it became the custom for commanders of fleets to receive a set of signals and sealed orders for their guidance, just before going afloat. The method was developed by the Duke of York, afterwards James II, who, as Admiral of England, introduced a system of signals for Divisions of Fleets as well as for single ships, whereby they could be directed in a specific manner. James II. was also the first to adopt scientific formations of line, and an order of battle. These instructions led to the British Naval Code. In France, Le Père Hoste, who wrote in 1697 a book on naval tactics, introduced in 1727 a system of signals by means of sails, flags, and gun-fire. Thus the advance of practical telegraphy was along two converging paths: (1) Visual transmission devices; (2) Codes. In the work of initiation, England and France were alike conspicuous, and history has fairly distributed the honours; for while Ignace Chappe himself frankly states that the nearest approach to the true principles of the art of telegraphy was made by Dr. Hook in 1684, Macdonald—who also lays stress upon the importance of the work of Hook—does whole justice to Claude Chappe, by giving credit to him for a great achievement in the development of "semaphoric wings". Tuck tells us that the idea of running a commercial telegraph system in England was almost realized in 1842, when one day there appeared on the Shot Tower, near St. Olave's Church, on the south side of London Bridge, the inscription: "Watson's telegraph to the Downs". Watson had bought up the old telegraph and semaphore stations, and he proposed to run them as a commercial speculation. Unfortunately, however, the semaphore on the Shot Tower was destroyed by fire on August 19, 1843, and it was not re-erected.

The condition of mechanical and optical telegraphy in England at the time when the electric telegraph was beginning to be established, can be gathered from a Parliamentary Return, now out of print, relating to the London-Portsmouth line.

ADMIRALTY SEMAPHORE, 24 *April*, 1843.

A Return of the Number of Hours in the Day appointed by the Admiralty Printed Instructions, dated 4th December 1827,

for the Ordinary Working of the Semaphore from London to Portsmouth.

From the 1st October to 28th February, from 10 A.M. till 3 P.M.	5 hours.
From the 1st March to 30th September, from 10 A.M. till 5 P.M.	7 hours.

Note.—Notwithstanding the above regulation, a look-out is kept at the several semaphores, and the closing sign is not made on any evening until their Lordships' commands are taken; those commands are never applied for whenever the sight is good until 4 o'clock P.M. in the winter, and 6 o'clock P.M. in the summer, and the line is kept open if required.

A Return of the Number of Days, during a period of Three Years, ending the 5th April, 1842, when the Semaphore was not available, by reason of the State of the Atmosphere.

Dates.	Admiralty, Number of Days.	Chelsea, Number of Days.	Putney, Number of Days.	Portsmouth, Number of Days.
From 1839–40 .	133	64	42	21
From 1840–41 .	106	70	49	28
From 1841–42 .	84	77	51	16
Total Number of Days at each Station for Three Years . . .	323	211	142	65

Note.—Whenever the working of the Semaphore at the Admiralty is prevented by reason of smoke, the State of the atmosphere to the eastward or westward from the vapour arising from the water in St. James's Park or other causes, messages are on such occasions, when of importance, taken to or from Chelsea or Putney stations, and generally effectually communicated from thence to or from Portsmouth.

(Signed) CHARLES H. JAY, *Commander, Superintendent.*

SEMAPHORE (LONDON TO PORTSMOUTH)

Return to an Order of the Honourable The House of Commons dated 6th April, 1843:—*for*

A Return of all expenses appertaining to the Semaphore from *London* to *Portsmouth* for the Three Years ending the 5th day of April 1842, including the Pay of the Officers and Men; also of the Number of Hours in the Day appointed by the Admiralty for the Ordinary Working of the same:—And of the Number of the Days during such Period of Three Years when the Semaphore was not available, by reason of the State of the Atmosphere.

H. F. AMEDROZ.
Chief Clerk.

ADMIRALTY, *May* 2, 1843.

Ordered by The House of Commons to be Printed, 5 *May,* 1843.

ADMIRALTY, 28 *April,* 1843.

A Return of all Expenses appertaining to the Semaphore from *London* to *Portsmouth,* for the Three Years ending the 5th April, 1842, including the Pay of Officers and Men.

	£	s.	d.	£	s.	d.
1839	3269	3	3			
Abate, Half-pay the Officers would have been entitled to if unemployed	1500	12	0			
				1768	11	3
1840	3293	19	6			
Abate, Half-pay as above . .	1514	15	0			
				1779	4	6
1841–42	3653	5	0			
Abate Half-pay as above . .	1558	11	6			
				2094	13	6
				£5642	9	3

Memorandum.—The above Return has been made up to the 31st March, the termination of the Financial Year of this Department, to which date the Accounts are rendered.

(Signed) J. T. BRIGGS,
Accountant-General of the Navy.

Fig. 1. Portrait of Francis Ronalds.

X

FRANCIS RONALDS

Although the dawn of electrical communication, near the beginning of the nineteenth century, was brilliant with the results of experiment, progress during the first thirty or forty years of the approach towards practical electric telegraphy was slow and hesitating. The chief reason for this was that the principles of quantitative research had not yet been ascertained. With a few exceptions, the laws of electrical circuits were imperfectly grasped, methods of electrical measurement were crude and sometimes false, and the equipment of laboratories was ill-designed for precision. To illustrate some of the conditions that then held, attention may be directed to an investigator who, from the vantage ground of experiment and bibliographical eminence, saw most of what was being revealed, who in the course of his long life maintained touch with progress at many points, and who assisted to record the advance.

Such a man was Francis Ronalds. In 1816 he devised and constructed a working electric telegraph that has frequently been designated the first. He possessed knowledge and experience wider than that of most of his contemporaries, and to these advantages he added the merit of unfailing fairness. For the invention of the electric telegraph he disclaimed credit. He prided himself solely upon the part he took in securing a practical result. Electricity had become a science, but not yet an engineering science. He bridged the gap that for so long separated tentative efforts from trials with circuits designed to work efficiently and to endure. His name is also to be honoured for collecting from every available source, at home and abroad, the books and pamphlets now famous as the Ronalds Library.

As the best account of him forms the Introduction to the Ronalds Library Catalogue, a brief history of the Library must take precedence of the story of his career. The Introduction to the Catalogue was written in 1880 by Alfred J. Frost, who was then Acting-Librarian of the Society of Telegraphic Engineers and Electricians. It contains more than 13,000 entries of titles and particulars of books and papers on electricity, magnetism, the electric telegraph, and other subjects. These entries relate to publications to be found in the Ronalds Library, as well as to all other works that came to the notice of Ronalds who compiled the list. The volume thus constitutes a Bibliography as well as a Catalogue. To-day the Ronalds Library contains 1965 bound volumes, and 250 bound sets of pamphlets. It was originally the intention of the compiler to present the books and papers to the Royal Society. Ultimately it was decided that they would be more useful in the hands of practical electricians and telegraph engineers. With this in view, he bequeathed the Library to his brother-in-law, Samuel Carter, who transferred it upon trust to what is now The Institution of Electrical Engineers.

Additional details have been found amongst loose papers and rough notes preserved in the Library. Some other particulars have been furnished by Mr. J. E. Montgomrey, to whom Ronalds was great-great uncle. The portrait (Fig. 1) is from a photograph lately brought to light by Mrs. T. C. Carter, widow of a nephew of Ronalds. Thanks are due to those who have in this way assisted, and also to the Secretary, to the Librarian, and to the Assistant Librarian, of the Institution of Electrical Engineers.

Francis Ronalds was born in London on February 21, 1788. His father, of the same name, was a City merchant. It is generally stated that the family resided at Brentford. From a letter written in 1860, from Padua, by Ronalds, to his brother-in-law, Mr. Carter, that has just become available, however, it is probable that during the boyhood of young Francis, the family changed their residence. He went to Dr. Phillips's Seminary at Walthamstow, where he acquired "a little taste for physics"—for Phillips had a good collection of instruments and apparatus.

The lad also obtained there "a smattering of geometry". He was transferred to the private school of the Reverend E. Cogan, at Cheshunt, "who hated Euclid as much as he loved Homer". It is to be surmised that not even Homer was given much attention; for, years afterwards, when writing his impressions of Sicily, Francis refers with regret to his own "inadequate classic knowledge".

At the age of fifteen, he was "consigned to a desk". The business in which he was engaged is not disclosed, but he says:

> My principal scientific studies were devoted to the service of a mouse-trap company. We succeeded very well until no mice remained to be caught.

At nineteen, he lost his father, and there devolved upon the young shoulders the management of a business with a turnover of about £150,000 per annum. The first year was successful. Thereafter, the combined actions of a speculating partner who ruined himself and died in a madhouse, "a disgust of trading habits", and "a penchant for other pursuits", soon caused Francis to steer a different course.

Very early in life, chemistry was his chief amusement, and a factor in his career; for "the blowing up of a large hydrogen gasometer in the breakfast room of No. 1 Highbury Terrace" was followed by the transfer of his "studio to a small cock-loft over the coach-house".

> Here, through a round hole, or window, in the south wall, I introduced one end of a long wire extended down the fields towards Holloway—not then built upon—and insulated it in a new manner, *called* Singer's (originally mine) on high poles. With this, with pith-ball-Electrometers, Bells, etc., I made a few unimportant observations of Beccaria's kind; and only mention them because I think that the idea of transmitting intelligence by discharging an insulated wire at given intervals first occurred to me whilst thus occupied. Mr. Cross (a patron of Singer) made experiments at about the same time, and in the same manner, on a very extensive scale, but I desisted, because the neighbours were occasionally affrighted by very loud detonations and said that they should be killed by "the Lightning which I brought into the place". In fact two or three of my neighbours were killed; but these were only un-

principled rats, experimented upon, dwellers in the Hay Loft, devourers of my poney's corn.

The date of those experiments is not explicitly stated, but it was prior to 1813; for in 1813 the home was at Upper Mall, Hammersmith (Figs. 2 and 3), where there was "a more elegant hay loft". At Hammersmith he fitted up a small apparatus with the addition of "registering electrometers". He explains that these are referred to in Singer's *Elements of Electricity*, and that there is "a slight mention of the Highbury (Holloway) wire in Brewster's Encyclopaedia", that "gave me a little encouragement".

The year 1814 was eventful for him. It was then that he became acquainted with his "kind old friend M. Deluc", and that he made experiments with Deluc's "dry column" to investigate "the still vexed question of chemical action and contact".

Deluc began his researches on the Voltaic pile in 1800. He published his *Traité élémentaire sur le fluid électro-galvanique* in Berlin, in 1806. In 1809 he sent two papers to the Royal Society, of which he became a Fellow. In 1810 he described in *Nicholson's Journal*, vol. xxvii. page 161, certain difficulties that arose in electrical experiments resulting from the "sticking" of the gold leaves of the electroscope to the sides of the instrument. He also constructed a voltaic "column" of six hundred "groups", and he observed the motion of a small bead of gold, suspended by a silk thread between two large brass balls. There was still "sticking". He then examined whether the "sticking" could be brought about by a mechanical impulse, but he found that this was not possible. In other words, he was observing the phenomenon known a century later as electrical *coherence*.

The next idea of Deluc was to obtain discharges between the smallest possible conducting masses. For this purpose he separated the brass balls considerably. He now suspended the gold bead by a very thin silver wire, which was allowed to swing into contact with a similar wire stretched tightly across a brass stirrup. This second wire was held at right angles to the suspension wire, about half-way up, and comparatively close to it, so that contact between the two silver wires occurred an

instant before the gold bead, in its swing, struck a brass ball. The jerk, produced at the meeting-point of the wires, prevented

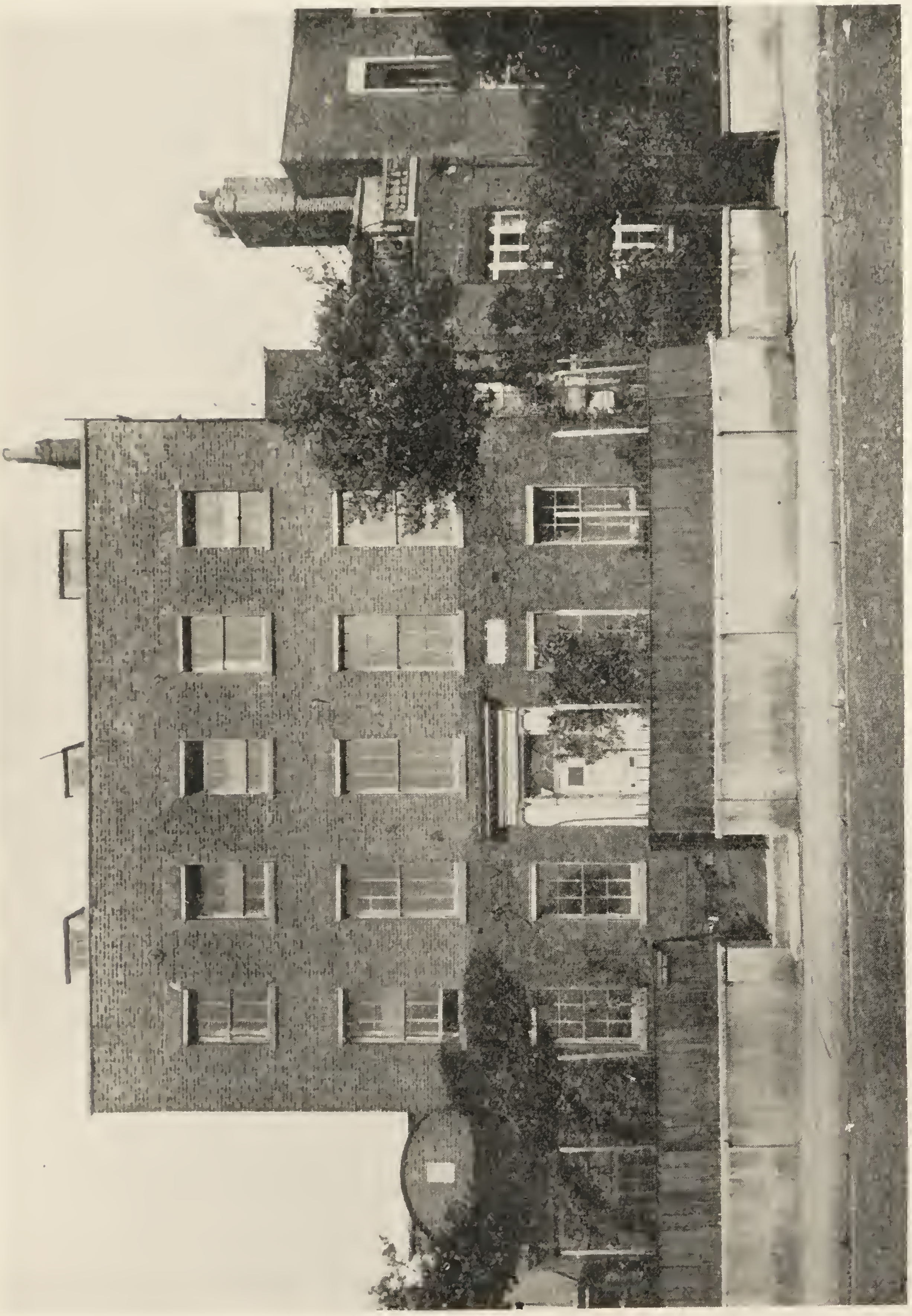


Fig. 2. The House, now known as Kelmscott House, at Upper Mall, Hammersmith, where Ronalds constructed and demonstrated his telegraph.

the "sticking" of the gold bead to the brass ball. By this means he obtained "strikings" that were regular and uninterrupted so

long as the apparatus itself was kept steady. Subsequently, he replaced the gold bead by a gilt pith ball, and this apparatus

FIG. 3. THE GARDEN in which Ronalds set up his telegraph and laid his cable, at Upper Mall, Hammersmith.

continued in motion for more than two years. It is described in vol. xlv. of the *Philosophical Magazine for* 1815, p. 261. These ex-

periments inspired Ronalds, who contributed an account of his work in this direction to the *Philosophical Magazine*, vol. xxiv., for June, September, and December 1814. In the same journal, on June 13, 1814, Mr. G. J. Singer refers to Ronalds as "an electrician of great promise, whose scrupulous attention to the essentials of accurate experimental inquiry it has frequently afforded me pleasure to observe".

The "dry column" used by Ronalds contained 1000 groups of zinc discs with interposed discs of "gold-paper". Measurements consisted in counting the number of "strikings" per minute of an electric discharger of pendulum form. Similar experiments were repeated with the "dry column" placed under an air pump receiver, the object being to examine whether moisture causes the discs to be more conducting, or whether it produces any changes "in the electrical state of the ambient air". He modified Deluc's apparatus by making a pendulum of inflexible wire instead of fine silver thread. With the help of a watchmaker he contrived a pawl-and-ratchet mechanism whereby the movements of the pendulum caused rotation of a pointer round a dial. He described this in a paper "On Electrogalvanic Agency Employed as a Moving Power", in vol. xlv. of the *Philosophical Magazine*, 1815.

Referring in later years to these early experiments, he claimed them to be "the first in which the distinction between quantity and intensity was observed", and he remarked that "these terms have been ever since much employed". Ronalds, in all other things modest, here took credit for having originated what, in effect, was afterwards denounced by others as a stumbling-block in the path to the understanding of electrical phenomena; for when, in 1841, the Royal Society bestowed the Copley Medal upon Ohm, the Council expressed their appreciation of Ohm's work, because it demonstrated "that the usually vague distinctions between intensity and quantity have no foundations, and that all explanations derived from these distinctions are erroneous".

The principal achievement of Ronalds was in 1816, when, to use his own words, "amid scoffs and jeers and a few imputations of insanity" he worked hard at the electric telegraph in

the garden at the Upper Mall, Hammersmith. The garden (Figs. 3 and 4) was 600 feet long.

The generator was a frictional machine of cylindrical form. At each end of the line was a pith ball electroscope (Fig. 5) that collapsed when the line was discharged. At the sending and receiving stations, respectively, were dials (Figs. 6 and 7) that were rotated synchronously by clockwork. The dials were marked with the alphabet, but were masked by circular front plates, each with a radial aperture that left only one letter

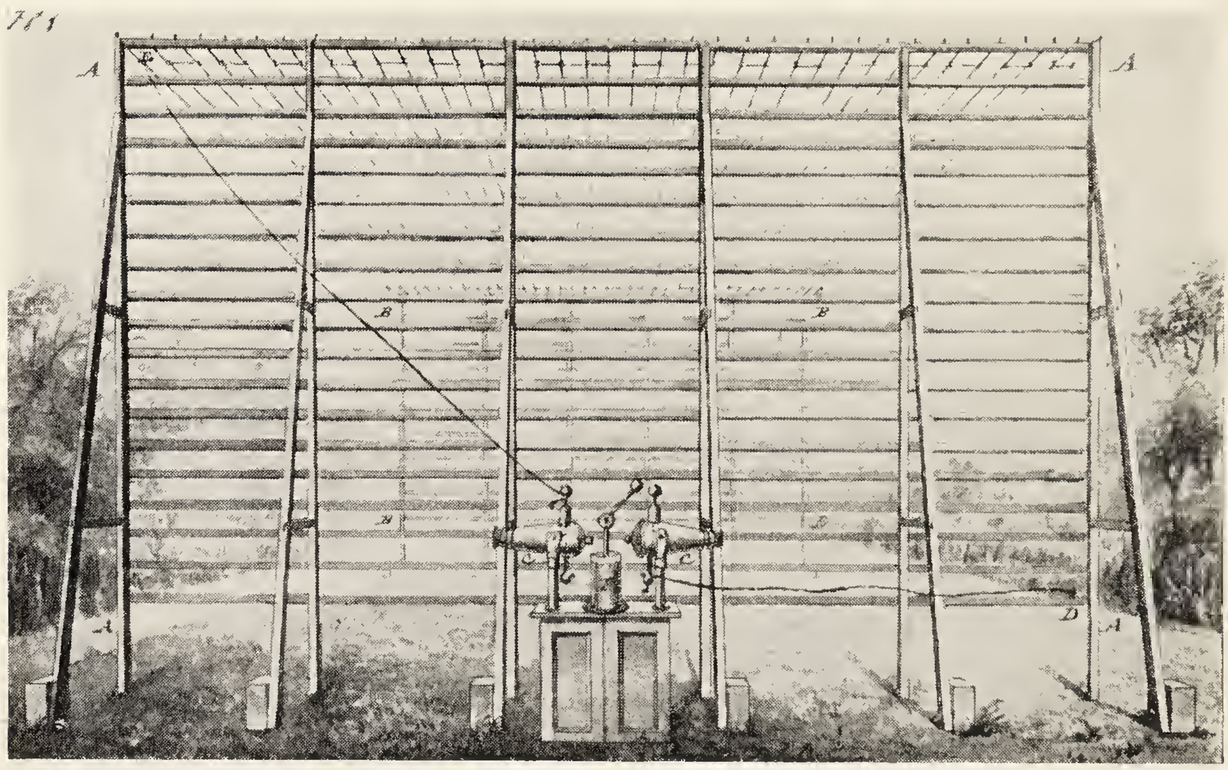


FIG. 4. RONALD'S TELEGRAPH, from a drawing found amongst his papers.

visible at a time. The sending operator discharged the line at the moment that the desired letter appeared at his disc. The receiving operator noted the same letter at his instrument when the pith balls collapsed. The first experiments were made through eight miles of overhead wire suspended by silk from hooks in horizontal bars (Fig. 4). He then proceeded to make his underground line. For this purpose a trench 525 feet in length, and 4 feet deep was dug in the garden. In this was laid a troughing of wood two inches square, lined inside with pitch. Within the trough were glass tubes, through which ran the conducting wires. Joints between the lengths of glass tube were formed by sleeves closed with soft wax. Wooden covers were

screwed down to close the trough, while the pitch was soft. The trough was then served with pitch and the whole was finally covered with earth.

In 1862 a portion of this cable and troughing was discovered in the garden at Hammersmith. It was placed, together with a copy of his book on the subject, in the Museum of the Pavilion

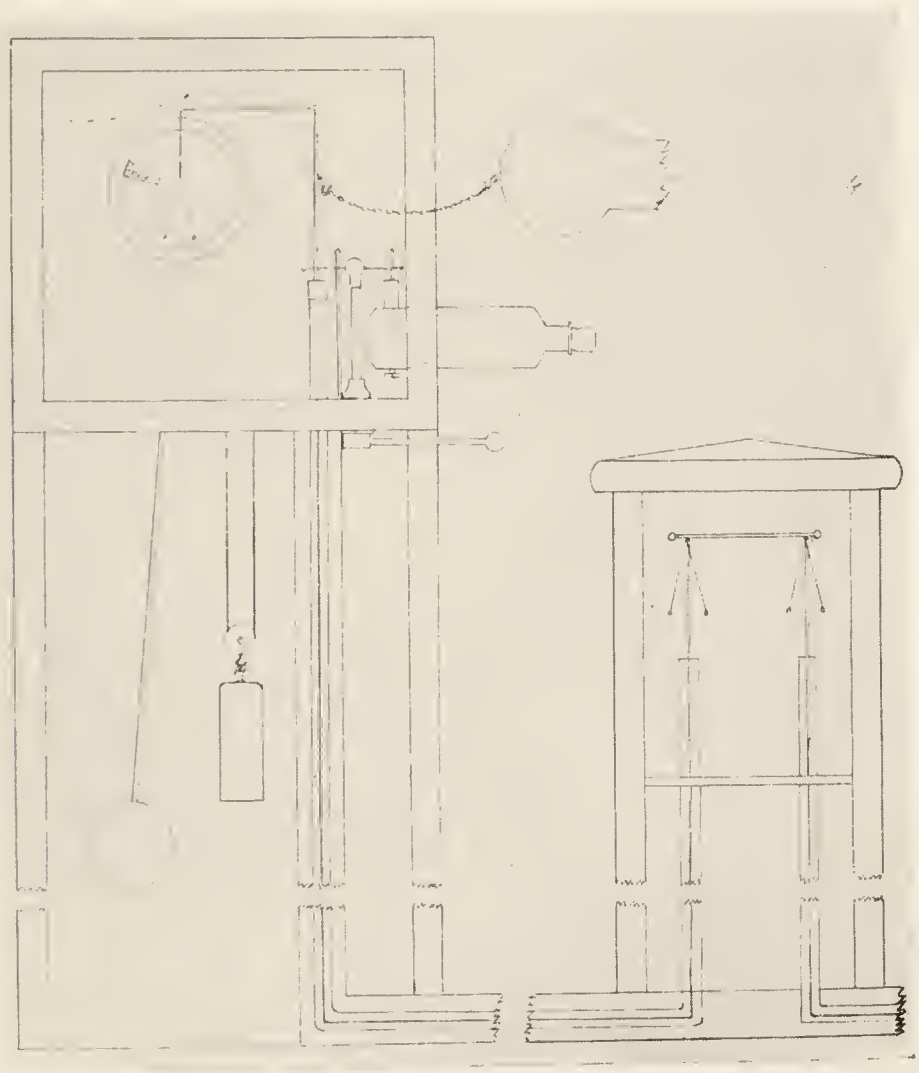


Fig. 5. Diagram, by Ronalds, of his telegraph.

at Brighton. A glass tube, or the greater part of one, with copper wire fitted, and one of the joints with a short glass tube that had formed a sleeve, were also found. The copper wire was in perfect order. The wooden trough and pitch had become consolidated with the earth. These relics passed into the possession of Latimer Clark. From him they were transferred to the Postmaster-General, and they are now at South Kensington. By the kindness of the Director of the Science Museum it has been possible to obtain a representation of them (Fig. 7).

The treatise by Ronalds entitled *Descriptions of an Electrical Telegraph and some other Electrical Apparatus*, was published in 1823. In it he first pays a tribute to "Dr. Watson and his friends" who in 1748 proved—by experiments upon a circuit of two miles, placed in a field near Shooter's Hill (Kent)—"that electrical shocks might be conducted through long circuits with immeasurable velocity". He does justice also to Volta, to Cavallo—who suggested "a method of conveying intelligence, by passing given numbers of sparks through an

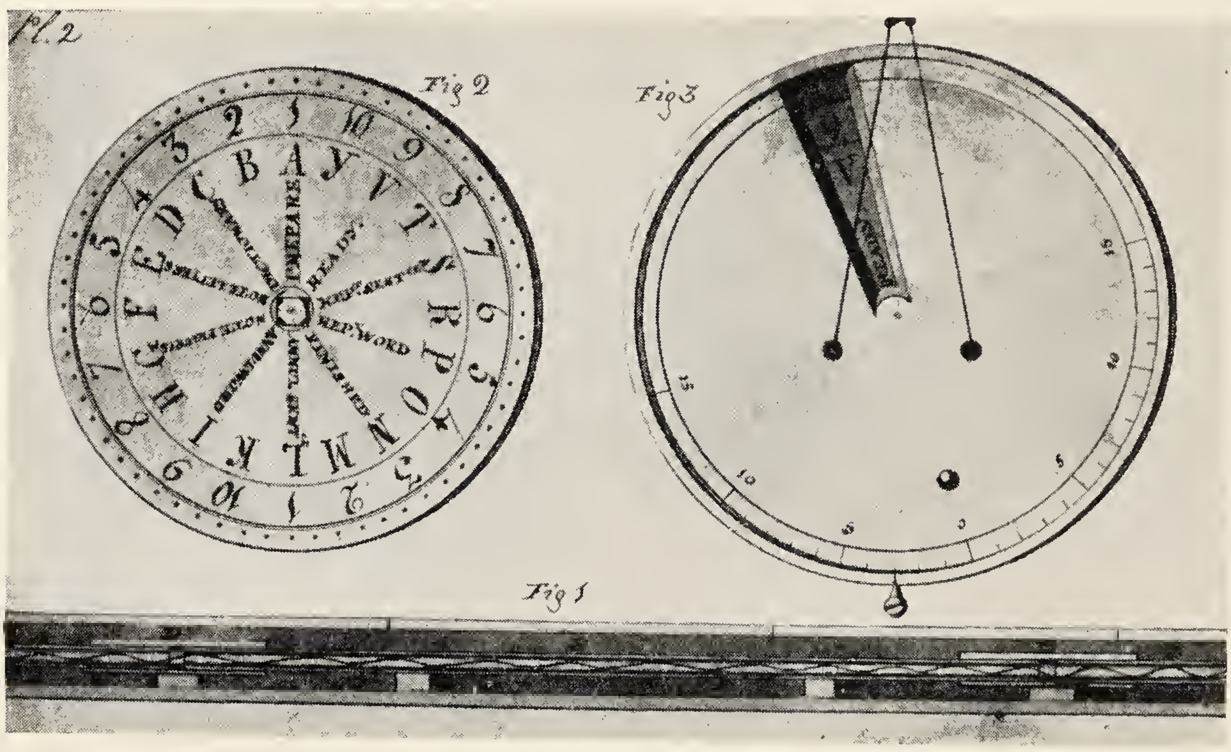


FIG. 6. DRAWING, by Ronalds, of the dial and cable of his telegraph.

insulated wire in given spaces of time"—and to those "German and American savants" who "first projected Galvanic or Voltaic telegraphs, by the decomposition of water". Then he states (p. 2) that

In the summer of 1816, I *amused* myself by wasting, I fear, a great deal of time, and no small expenditure, in trying to prove by experiments on a much more extensive scale than had hitherto been adopted, the validity of a project of this kind. I believe I succeeded to the entire satisfaction of several very eminent scientific friends; and I am sure that they will at least acquit me of wishing to claim the smallest share of originality which does not belong to them. Electricity . . . may be compelled to travel as many hundred miles beneath our feet as the subterranean ghost which nightly

haunts our metropolis . . . why has no *serious trial* yet been made of the qualifications of so diligent a courier? Why should not our Kings hold councils at Brighton with their Ministers in London? Why should not our Government govern at Portsmouth almost as promptly as in Downing Street? Why should our defaulters escape by default of our foggy climate? . . . Let us have *electrical conversazione offices*, communicating with each other all over the kingdom, *if we can*.

Fig. 7. Relics, found in 1862 at Upper Mall, Hammersmith, of the Ronalds telegraph of 1816. These were presented by His Majesty's Postmaster-General to the Science Museum, South Kensington, where they are now preserved.

The fact that most strongly impressed him in these experiments was the *instantaneous* transmission through eight miles. He did not contend or even admit "that an *instantaneous discharge*, through a wire of *unlimited* extent, would occur in *all* cases", but he was convinced that his experiments afforded "no grounds for abandoning the project of an electric telegraph". He added that "by the use of a telegraphic dictionary, a word or even a whole sentence, could be conveyed by only three discharges".

He invited attention to the discharge from a condenser "through a silken thread, or any similar substance, containing

a slight degree of moisture". He thought it might be used instead of "Voltaic streams" or electric sparks for the decomposition of water, and he remarked, "I have seen straw conductors in France, for the preservation of buildings, which have been adopted on this principle instead of points."

The problem of securing practically instantaneous transmission being solved, he next examined the question of durability, taking into account accident, wilful damage, duplication of routes, fault testing, testing-stations, inspection, generators of electricity consisting of static machines driven by steam engines "when the telegraph is at work", and insulation. He refers to glass "as being a substance more durable and *less* liable to accident than Mr. Cavallo's pitch and cloth; yet his method answered the purpose very well".

He met the objection of damage in time of war:

> Let us have *smokers* enough to prevent invasions, and Kings that love their subjects enough to prevent civil wars.

Smoker was a name given at that time to war vessels—originally fireships, or ships for smoke-screening. He definitely contemplated the use of watertight cast-iron troughs, and of duplicate routes for telegraph cables, for safety in war. To localize faults, he introduced "provers" at mid-distance, and he imagined "twenty proving stations between London and Brighton". His view was that

> Any sorry little two-penny post *cove* might take a canter on his Rozinantuolo, and, on his arrival at a prover, perform the operation on it in less time than I have employed to describe the manner of its performance. If he discover a fault, let him report his discovery accordingly to the engineer, who may open the trench and the trough. . . .

He knew how imperfect were the instruments—

> Any such continual flow (of electricity) may take place, and may even suffer changes in intensity, yet we and our imperfect instruments remain as little sensible to what is passing as our forefathers were to the motions of the earth or the pressure of the atmosphere.

His chief enemies were the *spiders*—

who frequently obliged me to walk a quarter of a mile, through wet grass, to destroy the webs which they had spun within the tubes . . . the moisture requiring as much time *to get out* of the series of tubes as it had taken *to get in*.

Ronalds never took out a patent for his telegraph. He offered his invention to the Government of the day. On July 11, 1816, he sent the following note to Lord Melville, who was First Lord of the Admiralty:

Mr. Ronalds presents his respectful compliments to Lord Melville, and takes the liberty of soliciting his Lordship's attention to a mode of conveying telegraphic intelligence with great rapidity, accuracy, and certainty, in all states of the atmosphere, either at night or in the day, and at small expense, which has occurred to him whilst pursuing some electrical experiments. Having been at some pains to ascertain the practicability of the scheme, it appears to Mr. Ronalds and to a few gentlemen by whom it has been examined, to possess several important advantages over any species of telegraph hitherto invented, and he would be much gratified by an opportunity of demonstrating those advantages to Lord Melville by an experiment which he has no doubt would be deemed decisive, if it should be perfectly agreeable and consistent with his Lordship's engagements to honour Mr. Ronalds with a call; or he would be very happy to explain more particularly the nature of the contrivance if Lord Melville could conveniently oblige him by appointing an interview.

UPPER MALL, HAMMERSMITH, *July* 11, 1816.

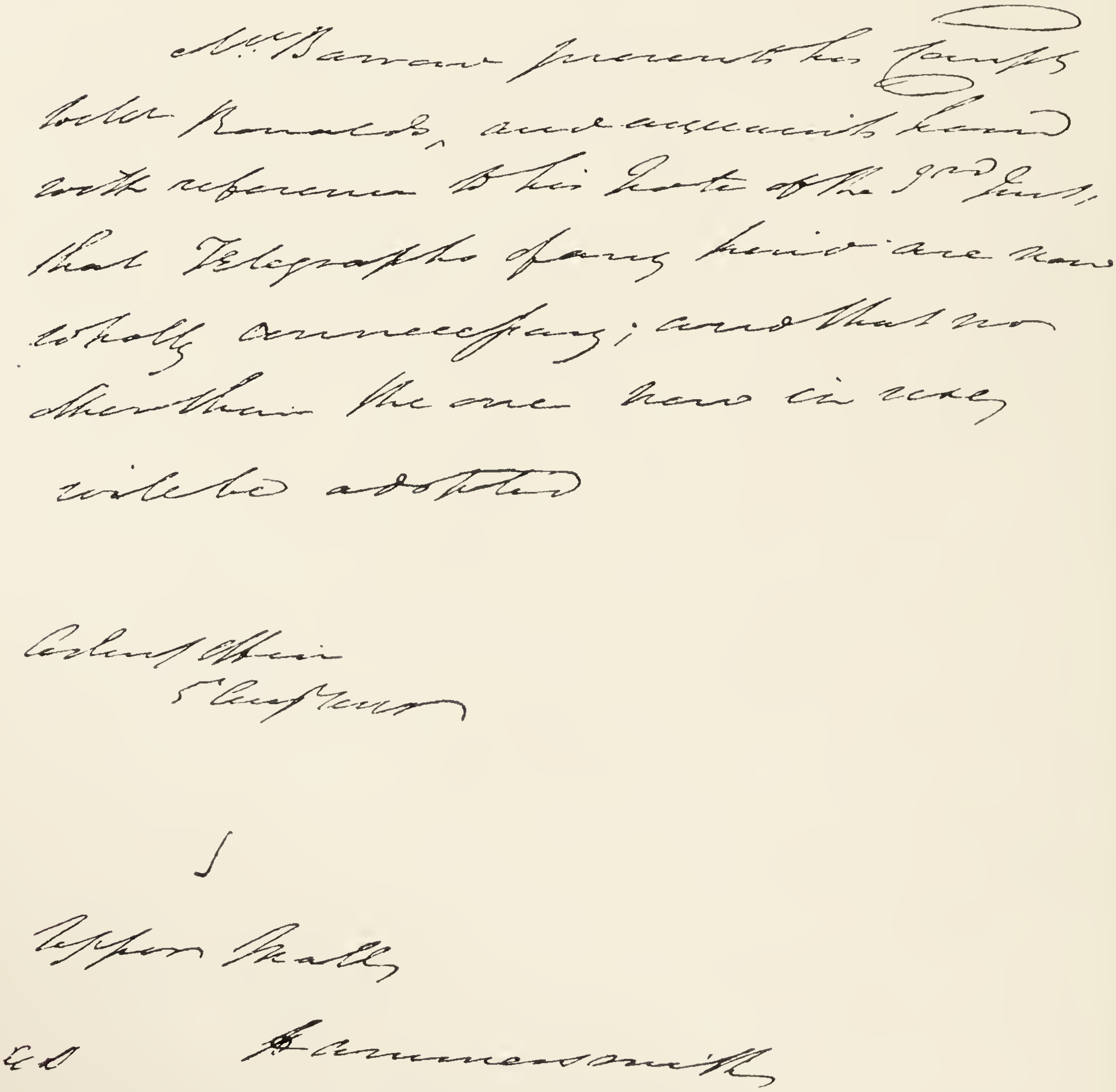
There was no interview. After further correspondence, Ronalds received a formal "acquaint" from the Secretary of the Admiralty (Fig. 8):

Mr. Barrow presents his compliments to Mr. Ronalds, and acquaints him with reference to his note of the 3rd inst., that telegraphs of any kind are now wholly unnecessary; and that no other than the one now in use will be adopted.

ADMIRALTY OFFICE, *5th August*, 1816.

In view of the tentative character of the apparatus, the action of the Amiralty in refusing it as a system to be installed as it was then presented was justified. The statement that telegraphs had become wholly unnecessary may have referred to the fact that there was a condition of peace. The wrong

consisted in the failure to recognize that Ronalds was offering something of sufficient merit to call for investigation and development. This defect, and the ludicrous conflict between what was wholly unnecessary and what was to be adopted, have


Mr. Barrow presents his Compts to Mr Ronalds, and acquaints him with reference to his Note of the 3rd Inst, that Telegraphs of any kind are now wholly unnecessary; and that no other than the one now in use will be adopted.

Admiralty Office
5 August

J

Upper Mall

Esq Hammersmith

FIG. 8. ACQUAINT, from Mr. Barrow, Secretary of the Admiralty, declining the electric telegraph of Ronalds.

caused the letter to be cherished as an example of bureaucratic intolerance and of graceless exercise of prerogative. The original is preserved in the archives of The Institution of Electrical Engineers. Ronalds remarks:

Lord Melville was obliging enough, in reply to my application to him, to request Mr. Hay "to see me on the subject of my discovery".

But before the nature of the discovery was yet known, except to his friends, he received the intimation from Mr. Barrow. He says:

> I felt very little disappointed, and not a shadow of resentment . . . because every one knows that telegraphs have long been great bores at the Admiralty.

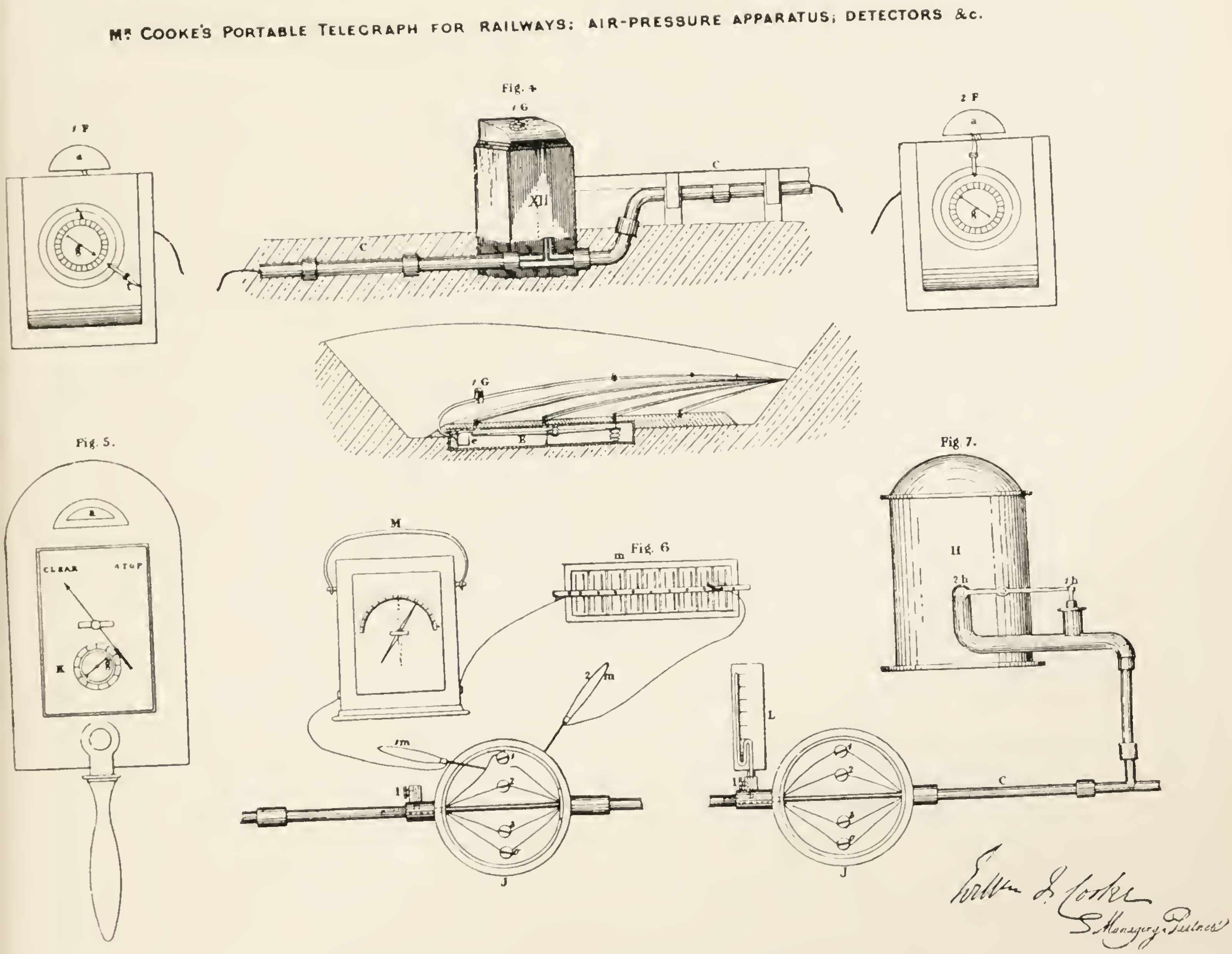


Fig. 9. Parts of Cooke and Wheatstone's Electric Telegraph and Cable. This drawing was published in 1840. Ronalds appears to have retained the copy to illustrate the similarity of the cable with that of his 1816 telegraph.

In 1817, Ronalds produced an electrical machine and other apparatus provided with spirit lamps for heating and thereby drying insulating supports. It has frequently been stated that in his telegraph he was the first to overcome the difficulties of insulation. He himself points out, however, that

readers of such works as those of Beccaria (1716–81), Lord Stanhope, Volta, Wilcke (1732–96), and Aepinus (1724–1802), ought to be aware of the facility with which conductors circumstanced as was his buried wire, could be insulated. His theory was that the buried wire served

. . . a like office to that of the interior coating of the Leyden Jar or of the shield of the Perpetual Electrophorus; which last is well known to retain a charge for *months* (aye years) although both its coatings may be in conducting communication with the earth (and with each other). The buried wire actually retained a part of the charge too long sometimes and even a small renovation of signs (due to certain effects of humidity I believe) sometimes occurred spontaneously. This was a slight but *real* objection, about equivalent, perhaps, to that arising from the retention of Magnetism in the electromagnetic Telegraph.

I am fully aware that what I have now said is open to discussion (but this is not the place for it) and am very far from advocating the cause of Static in lieu of *magnetic* (*sufficient* not *perfect* insulation could be attained)[1] electricity for telegraphic purposes; but I *will* say that if the electric telegraph of 1816 had been fairly examined, an *effective* instrument might have been in the hands of the Government and that after Dr. Oersted's experiments an *improved* telegraph might have been in the said hands; *also* that messages might have been conveyed thereby as *cheaply* in England, etc., as they are in America "a'most".

On some day before September 1818 Mr. Cooke's father a surgeon of Brentford, happened to visit us at Hammersmith, when I showed him and explained my telegraph. I believe he said that he would mention it to his son then in Germany. He was . . . Wheatstone's early partner. I became acquainted with Wheatstone in about 1820 I think (not having proper papers here—Padua —I cannot name the precise time).

Concerning this matter, Fig. 9 may be compared with Figs. 5 and 7. There is in the possession of the Institution of Electrical Engineers an original letter written—not in the best taste—by Cooke to Ronalds in 1870 that also bears upon this last paragraph:

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[1] And a provision was contemplated "for keeping up a sufficient and constant supply of electricity" (*vide*, Description of an Electric Telegraph).

Branksea Lodge, Tooting, S.W.,
London, *April* 1870.

My dear Sir Francis—Death has been heavy among my dearest friends and relations during the last fortnight, or I should have written sooner to congratulate you on Her Majesty's acknowledgment of your telegraphic labours.

It is singular that Brentford should have furnished two of the men who were most practical in their original views; I might say *the only two men* who up to the year 1837 realized in their minds the electric telegraph as a *future fact.* I have before said that you were before the time—had you taken the subject up again in 1835, it would have been all your own, I fully believe; or had we jointly taken it up in 1836, we should have divided the honours. At all events I am sincerely rejoiced that you are at last one of the number who have received national recognition. How many other names have preceded Wheatstone, Ronalds, and Fothergill Cooke which should have followed! Longo intervallo!

May you long be spared to enjoy the ephemeral honour, which, however, so granted, links your name with one of the greatest adaptations of science the world can ever know, next in usefulness to printing—in its character the most marvellous development of the Creator's powers vouchsafed to the practical service of man. To Him be the glory! To us the deep and abiding satisfaction of being His instruments.

Believe me, Very faithfully yours,

W. Fothergill Cooke.

Sir Francis Ronalds.

Cooke was no doubt right when he suggested that if Ronalds had adhered to his investigations of the electric telegraph he would have attained early to distinction. Why he discarded the work at that time it is difficult to explain. In the Preface to his *Descriptions of an Electric Telegraph and some other Electrical Apparatus*—written at Wigmore Street, London, on June 11, 1823, he confesses that he is ". . . taking leave of a science, which once afforded a favourite source of amusement" and that he is "compelled to bid a cordial adieu to Electricity".

Referring, in 1860, to this book he owns that it had a little success, "but it was written in great haste, and in love; and I went abroad again with a very unscientific object in view". That is the only hint to account for the break in his career somewhere between 1816 and 1823.

On August 31, 1818, he started by coach from London to Canterbury and Dover. He crossed, on September 2, to Calais —where he purchased snuff, a snuff-box, and a silk cravat— and he proceeded by diligence to Paris. Five days later he departed from Paris for Rome and Naples.

His route through Italy cannot be precisely traced, but there is evidence that he made experiments at Vesuvius upon the electrical condition of the atmosphere near the mouth of a crater, and that he made sketches to illustrate his investigation. In general, his letters at that time bear rather upon art than upon science. Writing from Naples to his brother, in January 1819, he reveals, however, that he has not entirely forgotten laboratory matters, for he says:

> It was singular enough that I fell in with Sir H. Davy at the Breakfasting Place, who had come there to shoot. I saw him have six fine shots running, but he discharges a Eudiometer better than a fowling piece, and missed all. He came with two Noblemen and another gentleman whose name I forget. If they were decent shots they must have required an extra horse or two to carry home their birds.

He left Naples, somewhat despondent, declaring that the carnival there was not half so good as Bartholomew Fair, nor was the ball so good as at Maynards. Similarly, he found "the *ton* of Milan far below that of Paris, and infinitely below that of London". His correspondence provides evidence of how great was the difficulty—when there was neither telegraph nor railway in Europe—to arrange for the transmission of cash. Banking arrangements were imperfect, and when he ran short of money he was placed "in the painful situation" of having to borrow from a comparative stranger. In 1819 it took twenty-five days for his letters to reach Naples from London.

He continued his journey by way of Tunis; but owing to plague being suspected, he was kept in quarantine for many days. He visited Sicily with Sir Frederick Henniker, and with him made a sketching tour. It was during this time that they both realised the need for mechanical sketching instruments—for as yet there were no cameras for travellers. Then he visited Egypt "where he got some teeth knocked out", he bathed in

Copy of a Letter to Mr. De Luc

My Dear Sir/

It seems ~~~~ a long time since I have enjoyed the pleasure of hearing either of you or from you. I now presume upon the Interest you have taken ~~expressed~~ in my pursuits to request your opinion (which if given will I am sure, be as candid as it will be estimable in every other respect) upon a scheme which has occurred to me and which I have been at some pains and expence in reducing to the test of experiment. It is shortly "A Method of conveying telegraphic signs by electrifying a Wire inclosed in a Glass Tube and buried at a small distance below the surface of high Roads.

I will give you as accurately as I can a sketch of the method which appears to me after several experiments the most eligible for putting this Idea into execution but if you should find the perusal irksome pray do not trouble yourself with it.

ABCD is a Wire which is extended from a Room next to my Study to another Room at the end of the Garden a distance of 525 feet, the part of it BC represents that buried in the earth 3 ft. deep inclosed in the glass tube. Each end of the Wire is furnished with a Cantons electrometer of Pith Balls on Wire thread H & I, F & G are two common Clocks placed immediately behind the pith Ball electrometers, they are made to go as nearly as possible synchronically and K & L are two dial plates which are capable of turning round their Centres, these beside being graduated in the usual way for Seconds (~~for~~ the seconds hands only ~~being~~ used) are marked with ~~a~~ letters of the alphabet at every space of 2 Seconds which leaving out U & J occupy 24 divisions, at the 25th division is written the word "Adjust ~~ed~~". at the 30th "Ready" at the intermediate divisions are written the words "Note letter" and "Note Figures".

To work my Telegraph

N.B./ Mr. De Luc died before this Letter was compleated

FIG. 10. DRAFT OF AN UNCOMPLETED LETTER FROM RONALDS TO DELUC.

the Jordan—"near which Sir Frederick got an ear nearly cut off"—and he went to Athens. He can be traced to Palermo, Cairo, Damietta, Jerusalem, Acre, Corinth, Patmos, Smyrna, Constantinople, and Malta. In his hours of ease he is discovered at balls, operas, theatres, and gaming houses. He even buys "punch for the party on Sunday".

In 1821 he devoted much time to drawings from his sketches, and upon a journal that never materialised. It was probably in that year that he returned to England.

From 1824 to 1828 he was devising perspective instruments. His book on *Mechanical Perspective* was published in 1828. It describes "an instrument for sketching from Nature accurately and conveniently every kind of object" and also "a machine for drawing in perspective architectural and other subjects, from measurement or from ground plans and elevations".

In 1836 there appeared his notes, in conjunction with those of Alexander Blair, concerning " Sketches at Carnac (Brittany)" and an account of the great *Roche aux Fées* of *Essé*. For this investigation he used his mechanical perspective apparatus which could be employed with "at least as great facility and dispatch as the Camera-lucida, and with very nearly as much accuracy as the sextant". He had the advantage of the results of the survey of the Carnac carried out by Mr. Murray Vicars in 1832.

When the Polytechnic Institution in London was first established in the Hay Lofts of the King's Mews—now the site of the National Gallery, near Charing Cross—Ronalds was asked to contribute towards its object. The exhibits which he placed at disposal for this purpose indicated the scope of his inventive genius: a new fore-bed for carriages; a new instrument for describing the Ellipsis and the Conchoids; an addition to the slide-rest used in turning curved surfaces; a semi-transparent sundial showing mean time; perspective instruments. Subsequently, the Polytechnic was removed "to near Cavendish Square", and Ronalds then added to the collection a self-acting fire-alarm which was afterwards transferred to Kew.

To exemplify the moral standard of some of the inventors of that time, he remarks:

A *gentleman* had another fire alarm there, and mine happening to be the best, the ticket on it was removed from the one to the other. A similar kind of manœuvre was executed at the great Paris Exhibition of 1855. The number, or ticket, of my Barograph was exchanged for a number tending to show that it was an invention of my rival Mr. Brooks; and curves, produced at the Radcliffe Observatory, Oxford, by my instruments were shown by M. Le Verrier at the French Institut, and taken by many for Mr. Brooks's curves. With the aid of the Abbe Moigno this *mistake* was detected and rectified.

Another invention of Ronalds's related to the anchoring of an observation kite. In 1847 he attached "three light cords to a kite and to three little stakes in the earth in an equilateral triangle". By this means the kite "was retained at a nearly constant elevation, whereas even small increments of the wind's force produce great depressions of the captive balloon". This was described in the *Philosophical Magazine* of September 1847.

In 1842 another complete break occurred in his career; he gravitated to the Kew Meteorological Observatory, which at that time was scarcely more than projected. The building in Richmond Park, formerly a Royal Observatory, was on May 26 of that year placed at the disposal of the British Association. In 1843 Ronalds was appointed its first Honorary Director and Superintendent. He held this post for nine years. His first step was to improve the apparatus and methods of measurement relating to atmospheric electricity.

Wheatstone was also concerned to some extent with the establishment at Kew, but his exact position was a matter of some delicacy. Amongst the correspondence is a letter from Sir Roderick Murchison, dated September 22, 1846, expressing regret that a portion of a paragraph in Murchison's Address to the British Association at Southampton, referring to the Kew Observatory, gave the impression that Ronalds's researches in atmospheric electricity had been suggested by Wheatstone. Murchison, subsequently correcting this, says:

The systematic enquiry into the subject of atmospheric electricity was entirely Ronalds's, but it was through Wheatstone's

energy and ability that funds were forthcoming to establish the Kew Observatory of the British Association.

This matter was put right in the final proof of the address. On February 2, 1844, Ronalds received an intimation from Wheatstone saying, "You were elected last night a Fellow of the Royal Society."

In addition to his telegraph, it was his adaptation of photography to self-registration of meteorological and magnetic observations that established Ronalds's fame. He began this work in August 1845 and applied the system to an atmospheric voltaic electrometer, a thermometer, a barometer, and a declination magnetometer. His first success in this direction was on September 24, 1845. An important paper by him concerning it was published in the *Philosophical Transactions of the Royal Society*, Part I., 1847, page 3. To assist him in the construction, he received certain grants from the Wollaston Donation Fund. On the other hand, although he was not a wealthy man, he bequeathed £500 to the Royal Society to augment that Fund.

The photographic records obtained by him were compared by Glaisher with corresponding simultaneous readings at Greenwich Observatory, and were found to be highly satisfactory. Sabine and others grasped the importance of this work and decided that after Ronalds had perfected his apparatus, similar equipment should be sent to Toronto "to record forces that may have acted many degrees apart but with an interval possibly of not more than 2 or 3 seconds" in magnetic storms. In 1846 Glaisher wrote from Greenwich that

> from August 7 to August 13, the motions of the two magnets (Kew and Greenwich) were similar in every respect and that your record of such motions is correct.

The relief to all concerned was intense. Similar photographic apparatus was then constructed and furnished to observatories at Toronto, Madrid, Sardinia, Oxford, Trevandrum, and elsewhere. The general appearance of it can be gathered from Figs. 12 and 13, for which acknowledgements are offered to the Director of the Science Museum, South Kensington.

Ronalds paid much attention to the electrical state of the atmosphere at Kew. His apparatus was constantly electrified during eight years, with a few very small intervals of transition from a positive to a negative state, and a few days for repairs. In five of these years, there were 15,170 observations. These were taken in conjunction with the observations of M. Quetelet

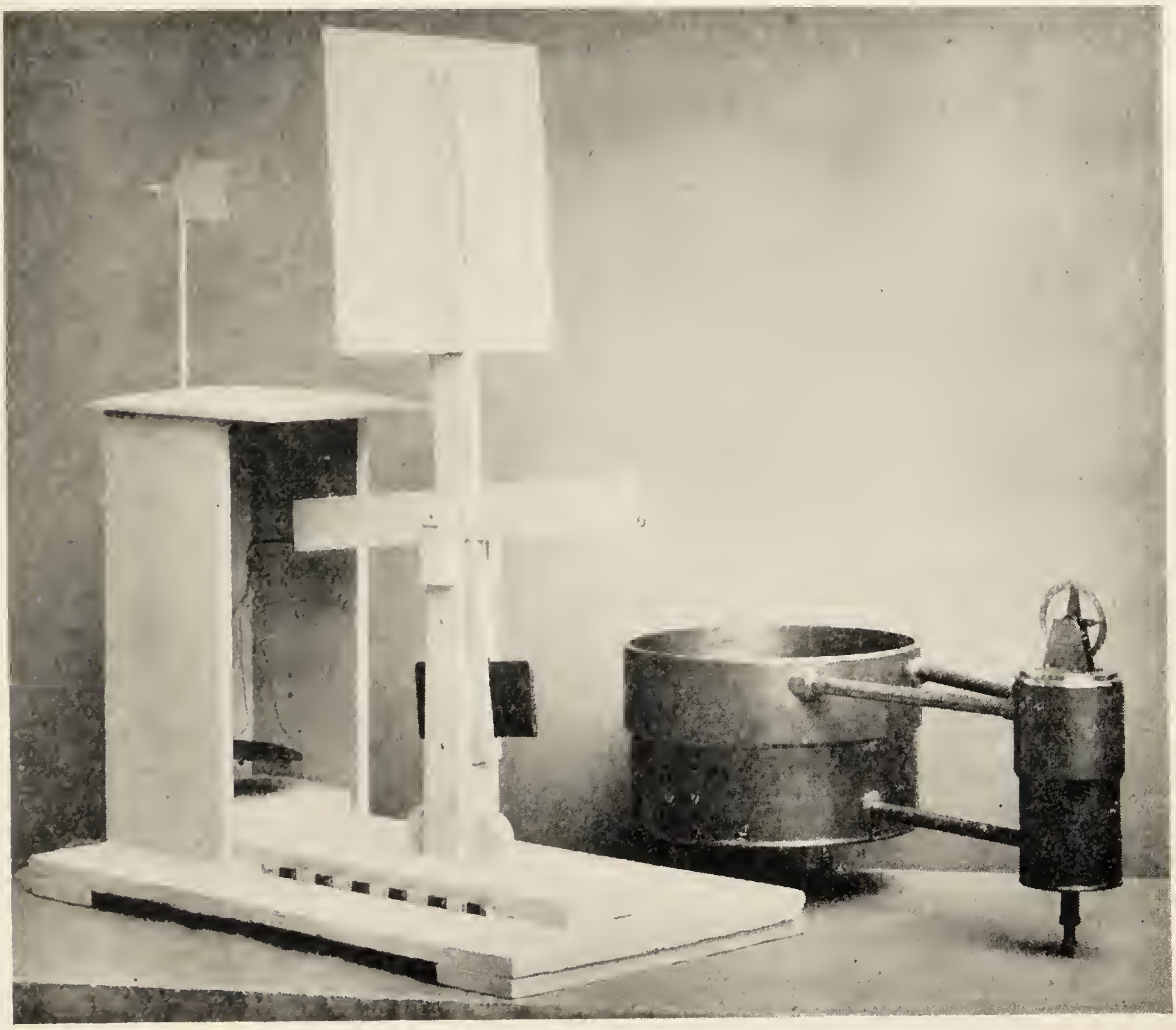

FIG. 11. RAIN-GAUGE AND WIND-GAUGE, designed by Ronalds; now in the Science Museum, South Kensington.

of the Brussels Observatory, and with those of Lamont of the Munich Observatory. Ronalds says:

> Had no *mean* molestations intervened in 1851–1852 I think I should have been enabled to elucidate a little the subjects of atmospheric electro-magnetism of the Aurora.

He mentions also his instruments described in the Kew Reports: an anemometer, a wind-vane (Fig. 11), an atmidometer (*i.e.* a vapour gauge), and a storm clock for facilitating the

registration of very sudden meteorological events. In 1848 he improved the construction of the declination magnetograph and made it applicable to the registration of the horizontal force "using Dr. Loyd's great amelioration of Gauss's system".

A sidelight upon these observations and upon Victorian generosity is afforded by a request from a Mr. Galloway of the Observatory, for an increase of salary:

on the ground that he is now obliged to observe from sunrise to 10 P.M., and that he will in future be required to observe from sunrise to 11 P.M.

The remark of Colonel Sabine is:

When Mr. Galloway's salary was increased from 1s. 6d. to 3s. per diem, it was distinctly understood that no further increase would be granted.

The Observatory at Kew, during Ronalds's tenure of office became a centre of attraction for men in every field of science. Evidence of this is to be found in the correspondence.

On November 8, 1845, Sir George Biddell Airy (1801–92), the Astronomer-Royal, wrote to say that he would send Glaisher to Kew to see the apparatus. On November 12, 1845, Airy wrote that the impression made upon Glaisher by the visit to Kew was such as would last to the end of Glaisher's life. He asked that the apparatus might be lent to Greenwich. There is also a letter from Professor Georg Adolf Erman (1806–1877), dated July 28, 1845. On December 13, 1845, Sir Charles Wheatstone introduced a horticulturist, Mr. E. Solley "who is engaged upon an historical account of the influence of electricity upon vegetation". In a bundle of loose papers there is also the following note:

September 4, 1846.

Dr. Oersted is very desirous of having the pleasure of making your acquaintance, and of seeing your apparatus at Kew, and proposes to be there on Monday at 1 P.M. I hope this will suit you. He will probably be accompanied by Mr. Forch-hammer.—Sincerely yours,

EDWARD SABINE.

On October 7, 1846, James Glaisher, who was then "Meteoro-

logical and Magnetical Superintendent" at the Royal Observatory, Greenwich, wrote to him with regard to the Murchison incident, and pacified him:

You know well what reverence I feel for the discovery, and the adaptations of photography to the self-registration of so valuable, yet so weak a force as is magnetism. Your photographs are certainly the best I have seen.

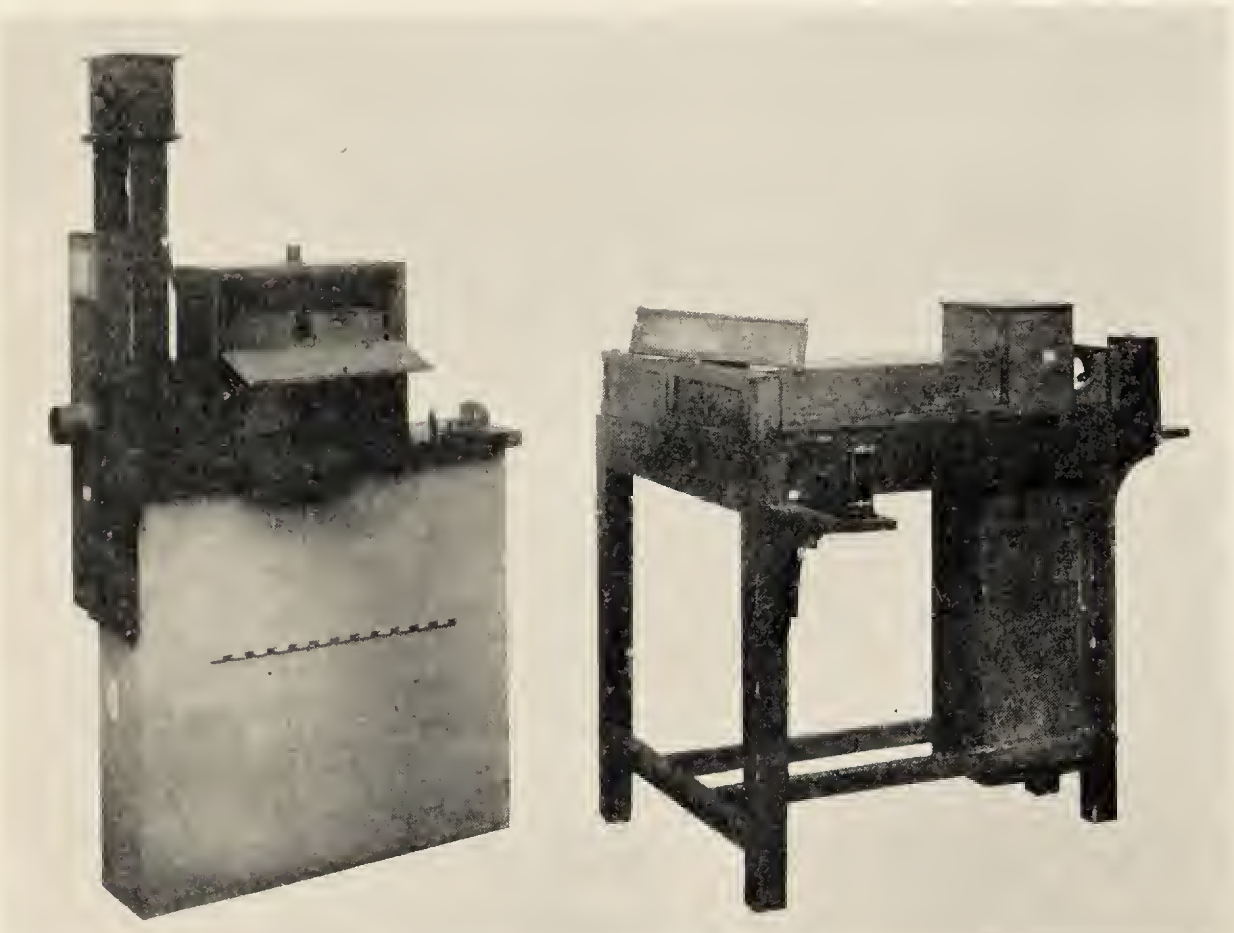

FIG. 12. PHOTOGRAPHIC BIFILAR MAGNETOMETER (left), and PHOTO-BAROMETROGRAPH (right), designed and constructed by Ronalds in 1847; now in the Science Museum, South Kensington.

On January 26, 1847, Airy from the Royal Observatory, Greenwich, wrote:

I shall be curious to see your barometer self-corrected for temperature arrangement. There is always a fear of failure in self-acting corrections of this character, but I do not know that it is well founded.

On February 12, 1847, Airy communicated with him again, mentioning the complication of the machinery, and his apprehension that it might go wrong. He adds: "the gridiron pen-

dulum is not often to be trusted, although it generally goes tolerably well".

Airy also objected that "the law of expansion of the metals is not accurately known and is not truly linear".

He therefore suggested that a record of temperature should be kept with all magnetic observations. Then comes what must have been a cheering note:

November 14, 1850.

To F. RONALDS.

I drop you a line to say that if the weather continues fine, which it promises to do, Mr. Faraday will visit Kew at the usual hour to-morrow, Sunday.—Sincerely yours, EDWARD SABINE.

To indicate some of the difficulties experienced by travellers before the days of the telegraph, the telephone, and the railways, it may be recorded that in 1848 Captain Younghusband, of Woolwich, who was the assistant of Colonel Sabine, desired to visit Ronalds at the Kew Observatory, and decided to proceed by water. He left Woolwich at 10 A.M. and did not arrive at Kew Bridge until 3.15 P.M. At Kew nobody could tell him which way to proceed to the Observatory. They insisted upon sending him to the Conservatory. The result was that he did not find Ronalds until 4.15 P.M. Younghusband declared that as the people of Kew all omitted the first syllable and spoke of the 'servatory, "it was not any wonder that there were difficulties".

The same trouble was encountered by another visitor, a Scandinavian, Mr. P. A. Siljestrom, who in 1848 wrote to Ronalds:

Your excellent Observatory seems to be more known anywhere else than in its neighbourhood. Indeed I was by no means able to find it, although I asked, I am sure, twenty people thereabouts, including several constables, who otherwise seemed to know every spot, but as for the Observatory, they obstinately directed me to the Conservatory.

A cunning mathematician, C. Tomkinson, in August 1848, failing altogether to discover it, returned to his home and prepared a chart, whereby he might try to locate it by latitude and longitude.

Some idea of what constituted electrical literature at that time is conveyed in a letter dated December 1, 1845, from Glaisher to Ronalds thanking him for the offer to lend him some books on electricity. He remarks: "The following are all that I have at my command—Priestley's History, Swinden, Bennett, Beccaria" (of which he had a new trans-

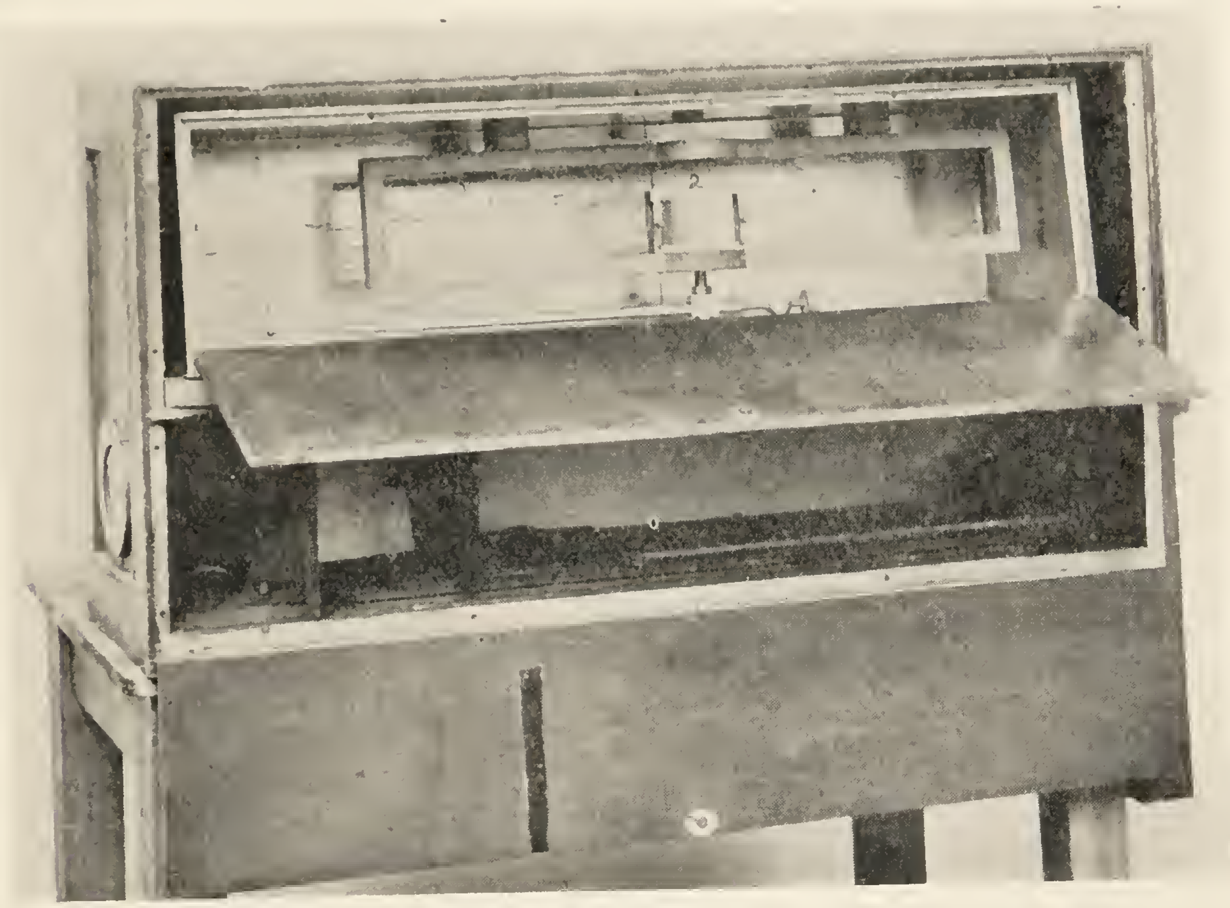

FIG. 13. MAGNETOMETER. The damping is supposed to be effected by the rectangular frame. This frame is of mahogany electro-plated with copper.

lation). "If there are other works, I shall be glad". Finally he writes:

Allow me to avail myself of this opportunity of thanking you for your kindness in assisting me in my pursuit of a better knowledge of Electricity.

The importance of the study of meteorology was emphasized on August 9, 1848, when great damage was done to the telegraph instruments at the Great Western Railway telegraph office at Paddington operating the line to Slough during a storm. It is recorded that there was an explosion in the office

"similar to a gun being let off". The coils of the instruments were fused. The same thing happened at Slough at the same time.

Among the correspondence, there is a letter signed by Alfred Weld, from Stonyhurst College, dated August 9, 1848, with regard to establishing an electrical observatory at that place. It describes the building of an octagonal tower for the purpose. A letter from James Glaisher, dated July 17, 1848, introduces Don Manuel Rico, who desired to obtain information for the establishment of an observatory at Madrid. There is also a valuable letter by George Fisher, dated December 27, 1843, relating to the conducting power of ice for electricity. A letter dated April (probably 1850) to Ronalds from Edward Sabine, informs him that "Sir (David) Brewster has got an idea of employing glass plates covered with white of an egg, instead of prepared paper or plates for photographs."

On April 6, 1850, he received particulars of "a meeting of gentlemen" at Hartwell House, near Aylesbury,

for the purpose of taking into consideration the present state of the science of meteorology, and to establish the British Meteorological Society with James Glaisher as Secretary.

At about that time experimenters were experiencing trouble in deciding upon a source of light for recording photographic work. Airy, on September 24, 1850, refers to using

the light of gas charged with naphtha vapour, in the manner in which we use it for the magnetic instruments, which will keep for weeks without requiring attention.

As an illuminating oil, Captain Lefroy at Toronto, October 8, 1850, obtained better results with winter-drawn spermaceti than with olive oil.

Ronalds regarded the "electrograph" as a far more necessary instrument than the barometrograph. The electrograph resembled part of a modern gramophone. It consisted of a horizontal circular plate of "cement composed of resin, bees' wax, and lamp-black". A circular vertical shaft, rotated by clock-

work, carried a rotating arm, from the end of which trailed a wire, terminating in a gold bead. The wire descended to the

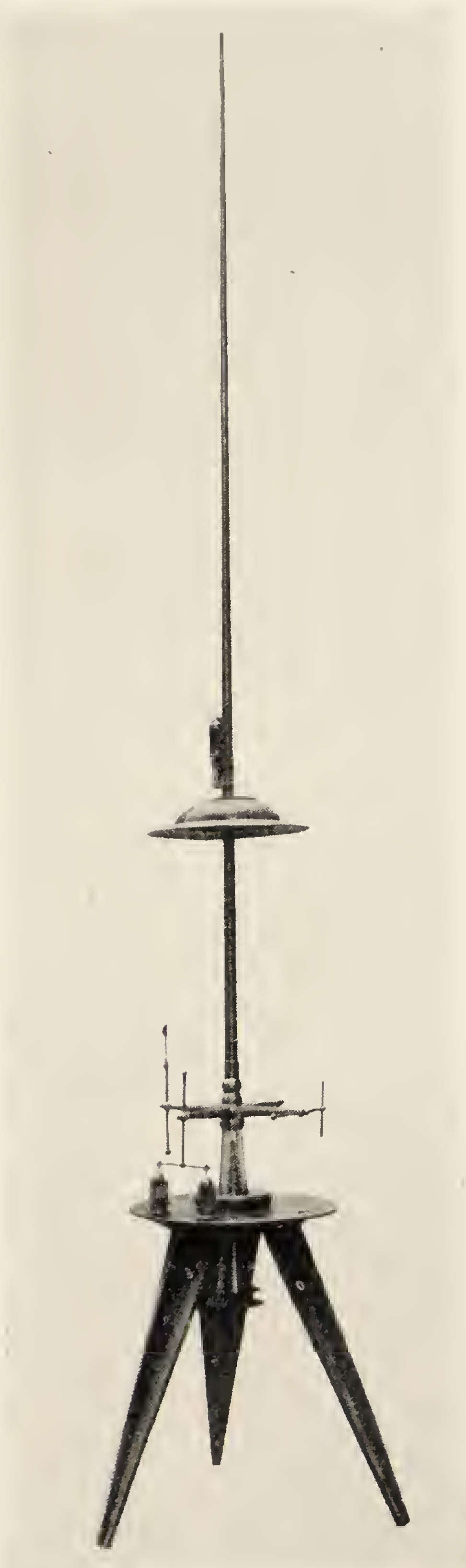

FIG. 14. FRANCIS RONALDS'S ATMOSPHERIC ELECTRICAL APPARATUS. (Total height, 19 feet.)

plate, and the bead glided in a spiral upon the surface of the cement. The spiral was formed by allowing the end of the wire

attached to the shaft to coil itself round the shaft as it rotated. Electrical contact was established with the bead upon the cement through a small mercury cup to which the wire was attached at the top of the shaft. Ronalds says:

> The gold bead acts upon the resinous plate like Mr. Bennett's electric pen, *i.e.*, it electrifies it in such a manner that when the plate is removed from the clock, and powdered with powdered resin, or even common *dry* hair powder, the line of the spiral exhibits configurations, which vary in form and width . . . and it is easy to discover the exact periods at which these occurrences took place . . .

For various reasons, in 1852 Ronalds became "annoyed and oppressed" at Kew, and he left his home at Chiswick. At about this time, his mother died. A small pension from the Civil List was awarded to him for "important discoveries in electricity and meteorology". In his letter of 1860 he says that he maintained the establishment at Kew against the strong opposition of many influential members of the British Association. He speaks in the highest terms of the support he received from Sir John Herschel, and of the liberality of the Royal Society—particularly also of its President, Lord Rosse.

In 1854 and 1855 he worked hard in the Imperial Library of Paris on the bibliography of electricity and magnetism. This labour began in 1815. With intervals of rest, he continued it, and the collection of books, until his death.

In 1854 and 1855 he prepared a short account of certain meteorological and magnetic instruments relating to the Kew apparatus. In those years, and in 1856, he might have been seen wandering into public and academic libraries and bookshops in any part of France or Italy gathering what he could to enrich the store of what he still called "dear old England". Extensive experience of this kind led him to declare at last that *bouquinistes* (second-hand booksellers) are "almost as difficult rogues as horse-dealers".

In 1860 he feared that he might not live long enough to arrange his collection of books. Happily, however, he attained to a great age. Towards the end, in his eighty-third year, on the recommendation of Mr. W. E. Gladstone, he was knighted by

Queen Victoria "in acknowledgment of his early and remarkable labours in telegraphic investigations".

He never married. He accumulated but little wealth. What he had, he bestowed upon scientific investigations. He died at Battle, in Sussex, on August 8, 1873.

The house at Hammersmith in which Ronalds lived is to-day in the possession of Morton Stephenson, Esq., through whose courtesy it has been possible to obtain the illustrations (Figs. 2 and 3). The residence is now known as Kelmscott House—a name given to it by William Morris (1834–96) who went to live there in 1878, and who there died on October 3, 1896.

William Morris—architect, poet, artist, novelist, and zealot, who possessed ideals as remote from those of Francis Ronalds as are the stars from the earth, was approached with a view to obtaining permission to place upon the house a memorial-tablet to Ronalds. The suggestion met with wrath. Morris declared that for their brutalizing influence upon humanity telegraphs were as much to be blamed as were railways. Fortunately, however, though the zealot thus objected, the poet conceded, and justice was done. The tablet to Francis Ronalds was affixed to the front of Kelmscott House with the full approval of Morris. It is seen to the left in (Fig. 1), on the wall of that "more elegant hay-loft"—the telegraph office of Francis Ronalds, that became the lecture-room where Morris sought to exert civilizing influence upon humanity by declaiming against the dehumanizing influence of civilization.

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discovers that cathode rays can pass through metals; publishes account of his discoveries, 130; his tribute to von Helmholtz, 131; estimate of his genius and work, 131-7; his long illness, caused by carious tooth, and death from blood-poisoning, 140; his amiability of character and modesty regarding his great discoveries, 140; considers application of Hertzian waves to telegraphy and telephony impracticable, 140. *See also* 12, 232, 235, 237, 238, 239, 240, 242, 248, 252
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